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quent life (mental as well as bodily) of each individual, with his course of action in the world, as a necessary consequence or resultant of these conditions—as strictly determined by his inherited and acquired organisation, and by the external circumstances which act upon it?

We must now consider what we understand by Man. I do not mean Man according to the zoologist's definition—a Vertebrate animal, belonging to the class *Mammalia*, order *Bimana*, genus and species *Homo sapiens;* but Man as he is familiarly known to us, and as we have to regard him in our present inquiry—the bodily man and the mental man. We cannot help separating these two existences in thought, although my own course of study has been directed to the investigation of the nature of their relation. The metaphysician considers man simply in his mental aspect; but he cannot help dealing with the organs of sensation, and the mode in which man acquires his knowledge of the external world through those organs; nor can he help dealing with the subject of voluntary action, and with the movements which express mental emotions. The physiologist, on the other hand, looks simply at the body of man; and yet he cannot help dealing with the physiological conditions of mental activity—the way in which we become conscious of the impressions made upon the organs of sense, and the mode in which the mind acts upon the muscular apparatus. A little consideration will shew that we may justly regard the body of man as the instrument by which his mind comes into relation with the external world. We all know that "I" means something distinct from the external world; and it is found convenient to call that personality by the Latin term *Ego*. This Ego—which feels, thinks, reasons, judges, and determines—receives all its impressions of the external world through the instrumentality of the body. Again, all the action of the Ego upon the external world—including in that term the minds of other men—is exerted through the instrumentality of the body. What am I doing at the present time?—endeavouring to excite in your minds certain ideas which are passing through my own. How do I do so?—by means of my organs of speech, which are regulated by my nervous system; that apparatus being the instrument

through which my mind expresses my ideas in spoken language. The sounds I utter, transmitted to you by vibrations of the air falling upon your ears, excite in the nerves with which those organs are supplied certain changes which are propagated through them to the sensorium, that wonderful organ through the medium of which a certain state of consciousness is aroused in your minds; and my aim is, by the use of appropriate words, to suggest to your minds the ideas I desire to implant in them.

Such is the aspect under which I would have you consider Man's body this evening. I do not say it is the only aspect: but it best suits our present discussion to consider the body as the instrument by which the mind of each individual is made conscious of what is taking place around him, and by which he is able to act upon the external world; thus becoming the instrument of communication between one mind and another. To illustrate what I would have you keep before you strongly—that the Mind is the essential Ego—I will ask your attention to one or two facts of very familiar experience. It must have happened to most of you to have formed impressions of other individuals without any knowledge of their bodily appearance. We do not know them in the flesh at all, but we know them intimately, or think we do, in the spirit. I remember, in the year 1851, the year of the first great Exhibition, being told that a number of the Telegraph establishments in the country having given their clerks a free ticket to London, to enable them to go up and see the world's fair—as it was called—in Hyde Park, almost every clerk on first coming to Town, before going to the great Exhibition, went down to the telegraph office in the city to fraternise with his chum. You probably know that telegraph clerks very soon find out who is at the "other end." Several clerks occasionally work a particular instrument, and each comes to know in half a dozen signals who has "gone on." They recognise the style of telegraphing, just as you would recognise the handwriting of a friend. After a little there is some one whom each comes to like better than others; A communicates individually with B, and B with A; and beginning with the exchange of little friendly messages at odd times, intimacies, I have been

assured, of the most fraternal kind, frequently spring up between those who have never seen each other. I daresay, now that young ladies are employed in telegraphing—and a most fitting employment it is for them—some more tender relations may spring up in the same manner.

Take again another illustration—the way in which our sympathies are aroused with an author, when we come to know his mind as presented in his writings. A great many of you felt when Dickens died, as if you had lost a personal friend—one with whose mind your own had grown into close relation, whose thoughts had exercised a most valuable influence on yours, and whom you felt to be nearer to you than many so-called friends.

Let me give you an instance from my own experience. I have been for some years a great admirer of an American writer, whose books I have read with the deepest interest, because I found in these books expressions of some of my own best thoughts, a great deal better put forth than I could put them forth myself—the products of a similar course of scientific inquiry, worked out with the aid of great poetic insight and a great fund of human sympathy, —a large human capacity altogether. In his writings I have felt as if I had one of my nearest and truest friends. Circumstances lately drew forth a letter from him to myself, in which he did me the honour to say that I had been his teacher in science; but I felt he was completely my master in everything that gives the best expression to scientific thoughts. Now if I were to go to America, the first man with whom I should seek to make acquaintance, with the certainty that we should meet as old personal friends, is Oliver Wendel Holmes.—I do not speak of Ralph Waldo Emerson, because we have long been personal friends. In the preface to a book I have lately received from him, he sums up all I have been now saying in these pregnant words— "Thoughts rule the world."

Thus it is the mind that reciprocates the mind, much more than the body reciprocates the body. The body is the symbol of the mind, just as spoken or written words are symbols of ideas; and when we think of a friend whom we know personally, we combine with the conception of his personality our whole knowledge and conception of his

character. When you say, "I met my friend so and so in the street," you do not mean you met simply his body, but that you met the man—the whole man. But when you say that you know a man "by sight" only, you mean that you know his outside body and nothing more.

In considering the body as the instrument of the mind, I shall shew you, first, the large amount of automatism in the human body, as to which I want you to have clear ideas. I do not wish, for any purpose whatever, to lead you away from this truth. I wish that you should be in the position yourselves to appreciate facts, so as not to be led away by one-sided statements. I desire particularly that my statements should not be one-sided; and so far as time will allow, I will place before you the whole of the most important considerations relating to this subject.

We must separate our body into two parts; and shall first consider the part that is most important as the instrument of our mind—that which physiologists call the *apparatus of animal life*. This takes in the nervous system—the recipient of impressions made by the external world upon our organs of sense, the instrument through which these impressions are enabled to affect our conscious minds, and conversely the medium through which our minds express themselves in action on our bodies. Then, again, there is the muscular apparatus, which is called into action through the nervous system, and the framework of bones and joints by which this muscular apparatus gives movement to the several parts of the body.

But this "apparatus of animal life" cannot be maintained in its integrity, and cannot perform the actions which it is adapted to execute, without certain conditions. It must be maintained by nutrition, because it is always wearing and wasting by its very action, and is in constant need of repair; and the material for this repair must be supplied by the blood-circulation. Again, the power it puts forth is dependent upon the operation of oxygen on the material of its tissues or of the blood which circulates through them; and this is as essential a condition as the pressure of water is upon the bellows of the organ.

Then the circulation of the blood involves the preparation of the blood from food, and its exposure to the atmo-

sphere in the lungs, so as to get rid of the carbonic acid which is the product of the chemical change that generates nervo-muscular energy, and may take in a fresh supply of oxygen; and hence there is required an *apparatus of organic life*. This apparatus consists of all the organs which take in the food, which digest it, prepare it, and convert it into blood, those which circulate the blood, and also those which subject the blood to the influence of the air. The working of this apparatus in man invôlves the action of certain nerves and muscles; though it is not so with many of the lower animals, which are provided with a much simpler mechanism. In the case of man we have the need of muscles to take in and swallow the food, and of muscles to move the coats of the stomach in the process of its digestion; and we require a powerful muscle—the heart —to circulate the blood through the body by the alternate contraction of its several chambers; while powerful muscles of respiration alternately fill and empty the lungs.

Now, the first point I would lay stress upon is, that all these actions are essentially and originally automatic. When I say originally, I mean from the very beginning— from the moment when the child comes into the world, or even before. We know that the first thing the new-born infant does is to draw a long breath; and from that time breathing never ceases,—the cessation of breathing being the cessation of life. The heart's action has been going on for months before birth; and its entire suspension for a very short time, whether before or after birth, would bring the whole vital activity of the body to an end.

These motions are executed by the nervo-muscular apparatus, in a way that does not involve our consciousness at all. We do not even know of our heart's action unless it be very violent, or we be in such a position that we feel it knocking against our side. But still it is going on regularly and tranquilly, though it may not be felt from one day's end to another. We cannot stop it, if we would, by any effort of the will; but it is affected by our emotional states.

So, again, we do not know that we are breathing, unless we attend to it. The moment that we direct our attention to it, we become aware of the fact; but if we are studying

closely, or listening to a discourse, or attending to some piece of music, or, indeed, doing anything that engages our consciousness, we are no more aware of our breathing than we are during sleep. This shews you, then, that when breathing goes on regularly the action is purely automatic. But we have a very considerable control over our muscles of respiration. If my respiratory movements were as purely automatic as those of an insect, I could not be addressing you to-night; because the whole act of speech depends upon the regulation of those movements. We must have such power over the muscles, as to be able to breathe forth successive jets, as it were, of air, which, by the apparatus of articulation, are converted into sounding words. Though we have power over the respiratory organs to a certain extent, we cannot "hold our breath" many seconds. In the West Indies the overworked negroes used formerly to try to commit suicide by holding their breath, but could not do it, except by doubling their tongues back so as to stop the aperture of the glottis; for the impulse and necessity for breathing became so imperative, that they could no longer resist the tendency to draw in a breath. Thus, whilst we have a certain voluntary control over this act of breathing, so as to be enabled to regulate it to our purposes, we cannot suspend its automatic performance long enough to interfere seriously with the aeration of the blood.

Let me briefly notice some of our other automatic actions. In the act of swallowing, which properly begins at the back of the throat, the "swallow" lays hold of the food or the drink brought to it by the muscles of the mouth, and carries this down into the stomach. We are quite unconscious of its passage thither, unless we have taken a larger morsel or something hotter or colder than ordinary. This is an instance of purely automatic action. If you carry a feather, for instance, a little way down into the "swallow," it is laid hold of and carried down involuntarily, unless drawn back with your fingers.

Take as another instance, the act of coughing. What does that proceed from? You may have allowed a drop of water or a crumb of bread to "go the wrong way," and get into the air-passages. It has no business there, and will excite a cough. This consists, in the first place, in the

closure of the glottis—the narrow fissure which gives passage to the air—and then in a sort of convulsive action of the expiratory muscles, which sends a blast of air through the aperture, that serves to carry away the offending substance. Nothing can be more purpose-like than that action, yet it is purely automatic. You cannot help it. You may try to stifle a cough for the sake of the audience or the lecturer, but the impulse is too strong for you. You see, then, the purely involuntary nature of this action. The person who feels inclined to cough may endeavour to overcome the automatic tendency by an effort of his will. He may succeed to a certain degree, but cannot always do so.

Now, although we cannot voluntarily stifle a cough when it is strongly excited, we can cough voluntarily, with no excitement at all. You can cough, if you choose, to interrupt the lecturer, as in the House of Commons coughing is sometimes used to put down a troublesome speaker; and little coughs are sometimes got up to give signals to some friend privately. Or, again, the lecturer, who may feel his voice husky in consequence of some little mucus in his throat, wishes to clear it away; its presence does not excite the movement, but he coughs intentionally to get rid of it. Now, I would have you fix your attention on these two points: in the first place, coughing as an involuntary movement excited by a stimulus in the throat; and in the second place, as a voluntary movement executed by a determinate effort. This distinction is the key to the whole study of the nature of the relation between the mind of man and his muscular apparatus.

The automatic movements of which I have been speaking depend upon a certain part of the nervous centres, which does not enter into the structure of the brain properly so called; namely, the *medulla oblongata*, or the upward prolongation of the spinal marrow—the spinal cord, as physiologists call it—into the skull (*a*, figs. 1, 2).

The effect of the stimulus or irritation in the windpipe may not be felt as tickling; for coughing will take place in a state of profound insensibility. An impression is made upon the nerves which go to the *medulla oblongata*, and in that centre excites a change. It is the fashion now to call

this change a "movement of molecules;" but it is nothing more than a name for the action excited there, of the nature of which we know very little. I do not think that this expression is really very much better than the old doctrine of "vibrations" put forth by Hartley more than a century ago. The change thus excited produces a converse action in the motor nerves which go to the muscles, and thus calls forth the combined muscular movement of which I have spoken. This is a typical example of what the physiologist terms "reflex action."

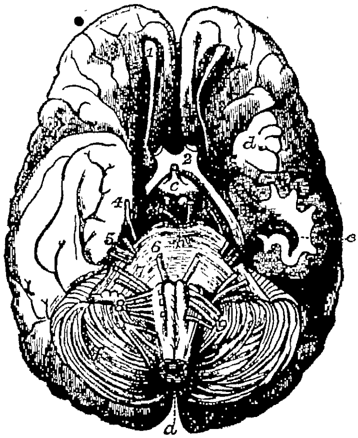


Fig. 1.—UNDER SURFACE OF BRAIN.—*a.* Medulla oblongata, cut off from the spinal cord; *b*, pons varolii; *c*, infundibulum; *d*, portion of the convoluted surface of the cerebrum; *e*, portion of the same laid open, shewing the difference between the grey or ganglionic substance of the convolutions, and the white or fibrous substance; *f*, cerebellum; 1, olfactory ganglion; 2, optic nerves; 3-9, successive cranial nerves.

The whole Spinal Cord is a centre of "reflex action," in virtue of the grey or ganglionic matter it contains, in addition to the white strands which form the connection between the spinal nerves and the brain; and this grey matter is present in different parts of the cord in different amounts, in proportion to the size of the nerves connected with each. Each ordinary spinal nerve contains both sensory and motor fibres, bound up in the same trunk, but these are separate at its roots (fig. 3); and a part of each set of fibres has its centre in the grey matter of the spinal cord itself, whilst another part is continued into its white strands. Although, however, we speak of "sensory" fibres, we do not mean that impressions on them always call forth sensations. For in the case of many involuntary acts, a certain impression is made on the sensory nerve,

and a reflex influence excited by this acts through the corresponding motor nerve without calling forth any sensation. An impression is conveyed towards the ganglionic centre, which possesses a power of *reflexion* — not reflection in the mental sense, but in the optical sense of the reflection of rays from a mirror. If we break any part of this "nervous circle," as Sir Charles Bell called it, its action is destroyed. Cut the sensory nerves, and no reflex action can be excited. Cut the motor nerves, and no muscular contraction can be called forth. Destroy the centre, and you will not have the reflexion. The complete nervous circle is necessary for the performance of every one of these reflex actions.

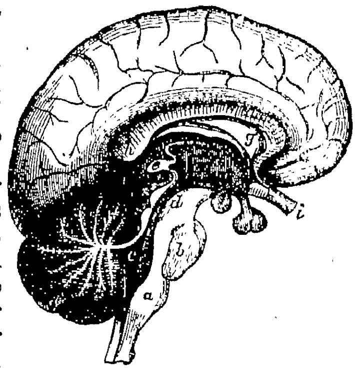


Fig. 2.—VERTICAL SECTION OF BRAIN THROUGH ITS MIDDLE PLANE; shewing the relation of the Cerebrum A and the Cerebellum B, to the Sensori-motor Tract, which may be considered as the upward extension of the medulla oblongata, *a*, and includes the parts lettered *d*, *e*, *f*; at *h* is shewn in section the corpus callosum, or great transverse commissure uniting the two cerebral hemispheres; and at *g* the longitudinal commissure, connecting the front and back parts of each; *i*, optic nerve.

What I want first to impress upon you is, that the reflex movements immediately concerned in the maintenance of Organic life all take place through this lower portion of the nervous system, which has no necessary connection with either sensation or will. That is to say, that if there were no higher part of the nervous system than the spinal cord, we should still have reflex action without the Ego having anything to do with it.

I may illustrate this by the act of sucking, which involves a curious combination of respiratory movements with movements of the lips. This act can be performed without any brain at all; for infants have come into the world without the brain, properly so-called—with nothing higher than

the prolongation of the spinal cord — and have sucked, breathed, and even cried for some hours; and all the true brain has been removed experimentally from new-born puppies, which still sucked at the finger when moistened with milk and put between their lips. This shews how purely automatic these actions are.

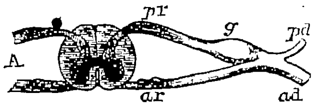


Fig. 3.—TRANSVERSE SECTION OF SPINAL CORD; shewing its grey or ganglionic core, enclosed in its white strands; *a, r*, anterior or motor roots; *p, r*, posterior or sensory roots.

But we now come to that other class of movements—namely, those properly belonging to the apparatus of Animal life—which are concerned in the obtaining of food and in carrying on ordinary locomotion. I have to shew you to what a large extent, among some of the lower animals, these movements are originally automatic; and, on the other hand, to inquire into their nature in Man.

We will go to the class of Insects and their allies the Centipedes, as giving the best illustration of the primary automatic movements of animal life. Here (fig. 4) is a diagram of a Centipede. Every child who has dug in the ground knows the "hundred-legs," and is pretty sure to have chopped one in two, and noticed that each half continues to run. This is in virtue of the ganglion existing in every joint of the body, which is the centre of the reflex action of the legs belonging to it, and which keeps each joint in motion even after it is separated from the body. If one of these creatures is cut into half a dozen pieces, every one of them will continue to run along. But, again, if we divide the nervous cord which connects the ganglia, the sight of an obstacle may cause the animal to stop the movement of its fore legs, yet the hind legs will continue to push it on. If you take out the middle portion of the chain of ganglia, the legs of that

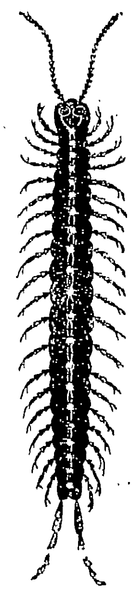


Fig. 4.—GANGLIATED NERVOUS CORD OF CENTIPEDE.

part will not move; but the legs of the front part will move or not, according to the direction of the ganglia of the head, which seem to control the action of the other ganglia in virtue of their connection with the eyes; and the legs of the hind part will continue to move as before.

When one of these creatures goes out of the way of an object before it, we may assume that it sees the object; for although we have no absolute proof that insects do see anything, I cannot see that there is any disproof of a conclusion to which all analogy points. Certainly it seems to me that if I try to catch a fly, and if it jumps or flies away, or if I go out and try to catch a butterfly with a net, and it flies off, it does so because it *sees* the net. Those who have watched bees, when a storm is coming on, flying straight down from many yards' distance to the entrance of the hive, can scarcely help concluding that they *see* the entrance. At any rate, it is not proved that they do not.

Well, then, the Centipede avoids an obstacle. A visual impression is made on the eyes, and by their agency is communicated to the large ganglia in the head; the reflex action of which controls that of the other ganglia, and *directs* the movement of the body.

We find that the size of these cephalic ganglia in flying Insects has a very close relation to the development of their eyes; the eyes being most highly developed in the most active insects, and the ganglia connected with them the largest; while the general movements of these insects are most obviously guided by their sight. Here is a clear case of original or primary automatism; because these actions are all performed by the insect almost immediately that it comes forth from the chrysalis or pupa state; as soon as its wings have dried, it begins to fly; and obviously sees and avoids obstacles just as well as if it had been practising these movements all its life.

Then, in the case of Insects, we notice that very remarkable uniformity of action, which we characterise as "instinctive." They execute most remarkable constructions after a certain plan or pattern, with such extraordinary uniformity and absence of guidance from experience, that we infer that they must have inherent in them a tendency to perform those actions.

We see this in the case of hive bees, which are distinguished for their elaborate architecture, and for their remarkable domestic economy. I do not say that there is no rationality in insects, and that there is nothing done with conception and purpose; because some of their actions seem to indicate this, especially those which are described in recent accounts of ants given by Mr. Belt in his "Naturalist in Nicaragua." Sir John Lubbock's experiments also certainly do seem to indicate a power of adaptation to changes of circumstances that were not likely to have frequently occurred naturally in the history of the race, so as to have become habitual—changes brought about by human agency, so foreign to the ordinary habits and instincts of the creatures, that we can scarcely attribute their consequent action to anything but a conscious adaptation to these ends. But this is a matter to be still cleared up—how far experience modifies the actions of insects. As a general fact, I may say that they carry Automatism to its very highest extreme.

To give another illustration—the *Mantis religiosa* (fig. 5), an insect which is allied to the crickets and grasshoppers, but which does not habitually either jump or fly. It is a very savage insect, and lies in wait for its prey like a tiger. You can see the curious form of the long fore-legs, which act as arms, and are waved about in the air; and it rests on the two hinder pairs of legs. Now, observe that the front pair are supported upon a very long first segment of the thorax; the two other segments bearing the wings and the two other pairs of legs. Each of these divisions has a ganglion, which is the centre of the movements of the limbs attached to it. The insect is always

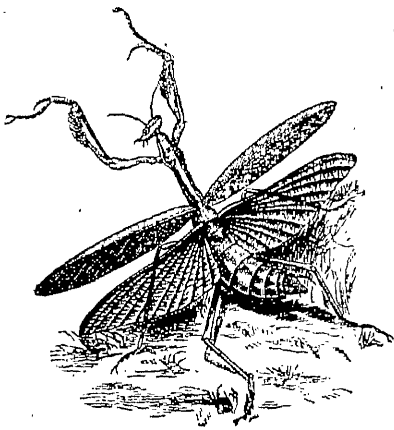


Fig. 5.—MANTIS RELIGIOSA.

lying in wait; and if any unlucky insect comes sufficiently near, the arms close round it and dig-in a pair of hooks, with which the feet are furnished. By this act the unfortunate victim is soon put out of existence. Now if the head of this Mantis be cut off, the arms still go on moving about in the same way; and if anything is brought within their reach, they impress the hooks upon whatever they grasp. The eyes simply direct their action, the action itself being dependent on the ganglion from which the nerves of these members proceed. Further, if we cut off that division and separate it from the hind part of the body, the same thing will go on. If anything is put within its grasp, the arms close round it and impress the hooks with just the same automatic action as we see in the Venus's fly-trap. Not only this, but if you try to upset the body, it will recover its balance, and rise again upon the hind legs.

This shews you how completely automatic the movements are. The name of *Mantis religiosa* is derived from the curious attitude in which this insect habitually lives—as if raising its arms in prayer. We have not this insect in Great Britain; but the French call it the *Prie Dieu*, which is equivalent to *religiosa*.

We now come to the lower Vertebrate animals, of which we may take the Frog as the best illustration. Its Spinal

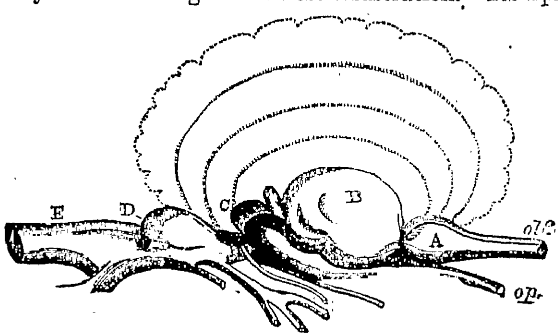


Fig. 6.—BRAIN OF TURTLE, with diagrammatic representation of the increased development of the Cerebrum in higher Vertebrata;—A, Olfactory ganglia; B, Cerebral hemispheres; C, Optic ganglia; D, Cerebellum; E, Spinal cord; *olf*, Olfactory nerve; *op*, Optic nerve.

Cord may be considered as the representative of the chain of ganglia in the centipede; the principal difference being that its ganglionic matter forms a continuous tract, instead of being broken up into distinct segments. But we find in the head, instead of the one pair of ganglia connected with the eyes, a series of ganglia connected with the several organs of sense, together with two masses of which we have no distinct representatives among the lower animals — namely, the *cerebrum* and the *cerebellum*. The relation of these to the other ganglionic centres is shewn in fig. 6, which represents the brain of the Turtle; A being the olfactive lobe, or ganglion of smell, from which proceed the olfactory nerves; B the cerebrum; C the optic lobe or ganglion of sight, from which proceed the optic nerves; D, the cerebellum; and E, the spinal cord. In most fishes the cerebrum is actually smaller than the optic lobes; but as we ascend in the series towards man, we find it becoming relatively larger and larger; so that it covers-in and hides the series

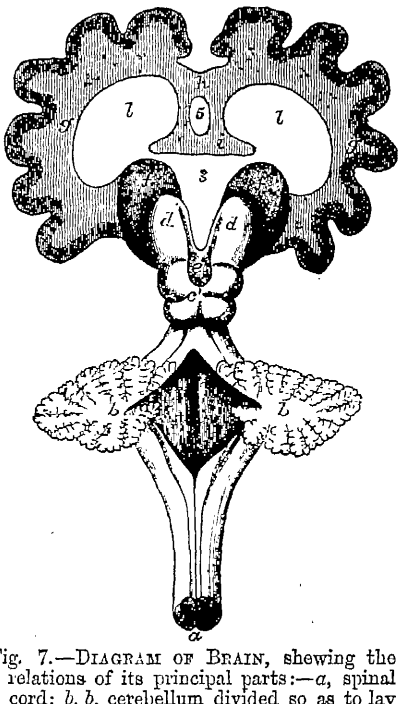


Fig. 7.—Diagram of Brain, shewing the relations of its principal parts:—*a*, spinal cord; *b*, *b*, cerebellum divided so as to lay open the fourth ventricle, 4, which separates it from the medulla oblongata; *c*, corpora quadrigemina; *d*, optic thalami; *f*, corpora striata, forming the sensori-motor tract; *g*, *g*, cerebral hemispheres; *h*, corpus callosum; *i*, fornix; *l*, *l*, lateral ventricles; 3, third ventricle; 5, fifth ventricle.

of ganglionic centres lying along the floor of the skull. These *sensori-motor ganglia*, (fig. 7, *c*, *d*, *f*), though commonly regarded as appendages to the cerebrum, really constitute the fundamental portion of the brain; they may be regarded as an upward continuation of the spinal cord; and I have been accustomed to designate this whole series of centres (excluding the cerebrum and cerebellum) as the *axial cord.* In this all the nerves of sense terminate, and from it all the nerves of motion arise, the cerebrum having only an indirect connection with either.

The proportional size of the Cerebrum in different animals, as compared with that of their axial cord, corresponds so closely with the manifestations of intelligence (that is, the *intentional* adaptation of means to ends, under the guidance of experience) as contrasted with blind unreasoning instinct, that there can be no doubt of its being the instrument of the reasoning faculty. The cerebrum attains its *maximum* size and complexity in Man; on the other hand, in the frog it is relatively much smaller than in the turtle; and it would seem that the actions of this animal are provided for almost entirely by the reflex power of its automatic apparatus—namely, the spinal cord with the ganglia of sense. Suppose that we divide the spinal cord in the middle of the back, between the fore legs and the hind legs, what happens? We find that the animal can no longer move the hind legs by any power of its own, but that they can be made to move by pinching the skin of the foot. If acid is put on one leg, the other will try to wipe it off; and a number of movements of that kind are called forth by stimuli of various kinds. Yet we feel justified in saying the frog does not feel them. We know, as a matter of experience, that if a man receives a severe injury to his back—as has happened very often in London, and also, I suppose, in Glasgow, among the shipping in the docks—through his striking some projecting object in falling, his legs are completely paralysed. He has no feeling in them, and no power of moving them. But after the first shock of the accident has passed off, if you tickle the soles of his feet, or apply a hot plate to them, the legs are drawn up. The man will tell you he feels nothing whatever, and would not know what had taken

place if he did not see the movement. A case of this kin occurred to the celebrated surgeon, John Hunter, who aske a man, "Do you feel this in your legs?" "No, sir," h replied, "but my legs do." That was not scientificall correct, because his legs could not be properly said to fee that of which the Ego was unconscious; but it expressec the fact that the irritation called forth a respondent motion

There is only one other mode of explaining this action namely, that by dividing the spinal cord we have made a second Ego—a new centre of sensation—in the lower part of the cord. In that case we make as many Egos in the centipede as we cut the body in pieces; and we might make three separate Egos in the frog—the head, the upper part of the trunk with the fore-legs, and the lower part with the hind legs, each acting independently. This seems to me inconceivable; I entirely go with those who maintain that these actions are provided for by a purely automatic mechanism.

A still more remarkable fact is, that if we remove the higher nervous centres, leaving only the Spinal Cord, and with it the Cerebellum (which appears to have the power of combining or co-ordinating the movements), we find that the general actions of locomotion are performed as in the uninjured animal. Thus the frog will continue to sit up in its natural position; and if we throw it into the water it will strike out with its limbs and swim, just as if the whole nervous system was intact. This is the case also with the *Dytiscus marginalis*, a water-beetle, which, when the ganglia of the head have been removed, will remain upon a hard substance without any movement; yet, if dropped into water, will begin to strike out, swimming in the usual way, but without any avoidance of obstacles. So the frog, if a stimulus is applied, will jump just as if the brain had been left. If put on the hand it sits there perfectly quiet, and would remain so unless stimulated to action; but if the hand be inclined very gently and slowly, so that the frog would naturally slip off, the creature's fore-feet are shifted on to the edge of the hand until he can just prevent himself from falling. If the turning of the hand be slowly continued, he mounts up with great care and deliberation, putting first one leg forward and then the other,

until he balances himself with perfect precision upon the edge; and if the turning of the hand is continued, over he goes through the opposite set of operations, until he comes to be seated in security upon the back of the hand. All this is done after the brain proper has been removed, shewing how completely automatic this action is. Another remarkable fact is, that if you stroke one particular part of the skin, the frog will croak.

Precisely parallel experiments were made by Flourens. By removing the brain of a Pigeon he found that the animal retained its position, and would fly when thrown into the air. If the optic ganglia were left, he found evidence that the animal either saw, or that its movements were guided by impressions received through its eyes. The head of the pigeon would move round and round if a light was moved round in front of the eyes. So in the frog it was found that, if the optic ganglia were left, it would avoid obstacles placed in front of it, when excited to jump.

Thus we see how completely automatic these movements are, and how entirely they are dependent on the reflex action of the axial cord, the Cerebrum not being necessary for their performance. The removal of that organ, however, seems to deprive the animal of all spontaneity; it remains at rest unless excited to move, and seems to do nothing with a purpose.

Let us now go to Man, and examine the nature of his movements. You have all seen a child learning to walk. You know that it does not get upon its legs to walk all at once, like a newly-dropped lamb; but that its muscles have to be trained, and this training is a very long process. The child learning to walk, as Paley says, is the greatest posture-master in the world. It requires a long course of experience to acquire the power of moving its limbs in a proper manner to execute the successive steps; but far more training is required in balancing. This balancing of the body is one of the most curious things in our mechanism. No automaton has ever been made to walk. I once saw an automaton that professed to walk; but it had only a gliding motion; and upon looking at the feet I found some concealed springs beneath, so that neither foot was ever really lifted.

The act of walking requires a continual shifting of the centre of gravity from side to side, so as to keep it over the base during every step; and it is this shifting from side to side, that constitutes the great difficulty in the act of walking. Almost every muscle in the body is in action in the maintenance of our balance and in the forward movement. The muscles of the eyes, even, are in operation in keeping our gaze fixed upon what is before us, and thus guiding our onward movement. But when this movement has been once acquired, it goes on unconsciously. If you are walking with a friend and engaged in earnest conversation, you may walk a mile and not be the least conscious all the time of your having been successively advancing one leg after another; and you do exactly the same thing while walking in a state of mental abstraction. So, again, you are guided by your sight, when you have once set out, along the line you are accustomed to take. I am in the habit of walking down the Regent's Park every lawful day, as you call it in Scotland, to my office at the University of London. I frequently fall into some train of thought—as lately about this lecture; and I follow on that train of thought, not only unconscious of the movements of my legs, but unaware of the directing action of my vision. Yet I know that my eyes have been directing me. When I have come into the crowded streets, I have not run against my fellow passengers, or knocked myself against a lamp-post. My legs have been moving the whole time, and have brought me to my destination, sometimes to my surprise. This must have been the experience of all of you who are accustomed frequently to walk along a certain line. It has even been the case that when you have set out with the intention of departing from your accustomed line, for some little business or other, and have fallen into a train of thought, through pre-formed association you keep in the habitual line. After getting half way down a street you suddenly find that you have not gone out of your way, as you intended to do. I regard such habitual action as purely automatic; not primarily, but *secondarily* automatic, the automatism not being original but *acquired*. This is the most universal of all forms of acquired automatic action

in Man—not only the motion of the limbs, but the direction of their movements by the sight.

The act of walking may become so automatic as to be performed during sleep. Soldiers fatigued by a long march continue to plod onward when sound asleep. If there are no obstacles they go steadily onwards, just like the centipede when its head has been cut off. The Indian punkah-pullers—men who are engaged the whole day pulling a string backwards and forwards, to move the great fan which produces a current of air in every room—often go on as well when they are asleep as when they are awake.

These are two instances of acquired automatism; and I might add a great many more, because everything that becomes habitual to a man is occasionally performed automatically in the state called absence of mind. Thus when a gentleman goes up to his dressing-room to dress for a party, the first thing he commonly does is to take out his watch and lay it on the table. The next thing he often does—I have done it myself—is to wind up his watch, as if he was retiring for the night. I have known a case in which the gentleman completed his undressing and then went to bed; so that when his wife came in search of him, he was comfortably resting from his day's work. That was a case of pure automatism; and I could relate many more instances of the same kind, but you must all have noticed such things in your own experience. A particular manual operation can be done, if it is one not requiring the constant direction of the mind, quite automatically. A man can plane a board, for instance, or work his loom, while his mind is entirely occupied in another direction. A musician will play a piece of music, and yet maintain a continuous conversation at the same time.

There is a very amusing and suggestive book which I recommend you to peruse, "The Autobiography of Robert Houdin, the Conjurer," who describes the training by which he prepared himself for the performance of various of his feats of dexterity. Amongst other things, he tells us that he devoted a great deal of time and attention in early life to the acquirement of the faculty of being able to read a book continuously, and at the same time to keep up balls in the air. He brought himself to be able to keep up four

balls in the air, without detaching his mind from his book for a moment. He could continue the train of thought that the book suggested, without giving his attention at all to the keeping up of the balls; this action being only a more elaborate form of the trained automatism that I have spoken of. The thought occurred to him, when writing his autobiography, that he would try whether, after thirty years' cessation from this performance, he could still execute it. He stops, and then continues his memoir: "I have tried this, and find I can keep up three balls." There, I believe, the nervo-muscular combination that was required, had come by early training to be a part of his physical constitution, and had been kept up by nutrition. Whatever, in fact, we learn to do in the period of growth, we can continue to do without practice after the growth has been completed; whilst acquirements that we make subsequently are more easily lost when we are "out of practice." I think all experience shews that; and I believe it is for this physiological reason—that the bodily and mental constitution acquired during the period of growth becomes "a second nature," and is maintained throughout life; whilst any modification it may undergo afterwards is something superadded to that basis, and is the first to decline when the habit of action ceases.

We now pass to the other part of our subject—the relation between the higher part of our nature, the Ego, and these automatic actions. What I shall endeavour to shew you very briefly is this, that the whole of the nervo-muscular apparatus concerned in executing the mandates of the mind acts as a trained automaton. Anything which we mentally determine to do "we will," as we say. In using the word "will" I do not mean a separate faculty, I mean the Ego in a state of action. The Ego determines to do a certain action, and commands the automaton to do it. The will does not, as physiologists used to believe, throw itself into a particular set of muscles; but says to the automaton, "do this," and it does it. There are many things which the Ego desires to do, but which he cannot make the automaton do for want of training. For instance, many of you may strongly desire to be able to play a musical instrument. You may be able to read the music, and by watching

a performer may see precisely how to do it, but you cannot do it, simply for want of training. The same is the case with a great many other actions which we can only acquire by practice. Again, you may wish to do something physically impossible. The Ego may earnestly desire and intend to make some great effort—to take a great leap, for instance, to save his life. He may will to hang on to a cord as long as may be necessary to prevent his falling from a height. The Ego wills this with all his energy; but his muscles will not obey him, because it is not in their nature to maintain their tension for longer than a certain period.

Let me give you a little experiment that I think every one will find instruction in performing on himself; it occurred to me while lecturing on physiology as suited to conduct my students exactly to the idea I wished to impress upon them. There happened to be a bust opposite me, and I said, "Now, I will to look at that bust, and I will at the same time to move my head from side to side." I told them to watch my eyes, and they could all see them rolling from side to side in their sockets,—as you can see for yourselves by looking at your own eyes in a looking-glass, and turning your head from side to side. You do not feel that you are using the slightest exertion, and would not be aware of the motion of your eyes unless you knew it as a matter of fact, or some one else told you that you were doing so. You have said to your automaton, "Look at it" (whatever it may be), and at the same time "move your head round;" and the automaton rolls its eyes in the contrary direction, and thus keeps the image on the same part of the retina.

That is what I maintain to be the general doctrine of the automatism of the body, directed and controlled by the will;—the Ego willing the result, and leaving it to the automaton to work it out; as when I set my automaton to walk to a certain place, and direct my thoughts to something altogether different.

We have now, in the last place, to consider how far the Mind of man acts automatically. This is a subject confessedly of very great difficulty. There are those who consider that the mind of man is essentially and entirely dependent upon his bodily organisation, although they may

still hold the separate existence of the mind. They find it, indeed, very difficult to conceive that there can be anything else than automatic action; because they see to what a very large extent our mental activity is conditioned by the physical constitution of the body.

The Physiologist can have no more doubt that there is a mechanism of thought and feeling, of intellect and imagination, of which the Cerebrum is the instrument, than that there is a mechanism of instinct of which the Axial Cord is the instrument. When one idea suggests a second, in accordance with a preformed association, the second a third, and so on, constituting what we call a "train of thought," without any order from ourselves, we seem fully justified by a large body of evidence in affirming that this is the mental expression of a succession of automatic changes, each causing the next, in the ganglionic matter which forms the convoluted surface-layer of the Cerebrum. These changes may or may not result in bodily motion. What we call the "movements of expression," are the involuntary signs of the state of our feelings; and so the movements executed by sleep-walkers are the expressions of the ideas with which their minds are possessed. So great talkers, like Coleridge, sometimes run on automatically, when they have got patient listeners; one subject suggesting another, with no more exertion or direction of the will than we use in walking along a course that has become habitual. All this may be regarded, physiologically, as the "reflex action of the cerebrum," the physical mechanism of which is partly shaped by its inherited constitution, and partly by the training to which it has been subjected, whether by intentional education, or by the education of circumstances—the brain "growing to" the mode in which it is habitually worked, just as the mechanism of our bodily movement shapes itself to the work we habitually call on it to perform. We constantly see that mental faculties are inherited, as well as bodily powers; that children brought up after the parents' death, shew most remarkably the mental tendencies of one or both of them. They do a number of things in exactly the same manner that the parent did, have the same moral and intellectual tendencies, and present an extraordinarily striking resemblance in general character.

This principle of the hereditary transmission of faculties through the physical organisation is now generally admitted; and what is more, I think it is clear that many of these faculties and tendencies have been acquired and superinduced, as it were, in the constitution of the parent, upon what it originally possessed. There is one very remarkable and too common example of this hereditary transmission, namely, the tendency to alcoholic excess. I remember a friend telling me he had known a man who for forty years got up every morning with the strong apprehension of being unable to resist that craving, which was an essential and inherent part of his nature, inherited from the unhappy indulgence of his father. That man fought a most heroic fight every day of his life. Every now and then he fell, but recovered himself; and, to my mind, fall as he did, his recovery shewed him to possess a far higher moral nature than that of the man who never yields because he is never tempted. I cite this merely as one example of acquired tendency hereditarily transmitted; all of us are familiar with cases more or less resembling it.

But the question is, whether the Ego is completely under the necessary domination of his original or inherited tendencies, modified by subsequent education; or whether he possesses within himself any power of directing and controlling these tendencies? It is urged by some that as the physical structure of his Cerebrum at any one moment is the resultant of its whole previous activity, so its reflex action, determined by that physical structure, must be really *automatic;* the only difference between a *voluntary* or *rational,* and an *involuntary* or *instinctive* action, lying in the complexity of the antecedent conditions in the former case, as distinguished from their simplicity in the latter. And it is held, in like manner, by many who look at the question from the mental side, and who do not trouble themselves at all about the physiological aspect of it, that a man cannot act in any other way than in accordance with his character; and that his character at any one moment is the general resultant of his whole previous mental life. But even John Stuart Mill, the most able and conspicuous advocate of this doctrine, felt that in making every man entirely dependent upon his in-

herited constitution, and his subsequent "circumstances," it excluded all possibility of real *self*-direction, all hope of *self*-improvement; and this, he tells us in his autobiography, weighed on his existence like an incubus. "I felt," he says, "as if I was scientifically proved to be the helpless slave of antecedent circumstances, as if my character and that of all others had been formed for us by agencies beyond our control, and was wholly out of our own power." The way out of this darkness he found in what seems to have struck him as a new discovery, although it was familiar enough to many who had previously studied the action of the mind,—"that we have real power over the formation of our own character; that our will, by influencing some of our circumstances, can modify our future habits or capacities of willing."

Now, this I hold to be accordant with the experience of every one who has thought and observed, without troubling himself with philosophical theories. We all perceive that in the earlier period of our lives, our characters have been formed *for* us, rather than *by* us. But we also recognise the fact, that there comes a time when each Ego may take in hand the formation of his own character; and that it thenceforth depends mainly upon *himself* what course its development shall take,—the most valuable result of early training being that which prepares him to be *his own master*, keeping in subjection his lower appetites and passions, and giving the most favourable direction to the exercise of his higher faculties. And I shall now explain to you what seems to me the process by which this is effected.

Every one knows that he can determinately *fix his attention* upon some one object of sense, to the more or less complete exclusion of all others. In looking at a picture, for instance, he can examine each part of it separately; or, if he has a "musical ear," he can single out any one instrument in an orchestra, and follow it through its whole performance. Now, just in the same manner we can fix our attention upon one state of consciousness (a thought or feeling) to the exclusion of others. Supposing that you are endeavouring to fix your mind upon a certain object of study, or are reading a book that requires much

thought to follow it, or are trying to master a mathematical problem, or are desiring to work out a certain question as to the conduct of your own lives, and you are attracted by the coming-in of a book or a newspaper which you would like to look at, or are distracted by noises or the playing of a musical instrument, you feel that it is in your power to fix and maintain your attention by a sufficient effort. That determinate effort is what we call an act of the will; and I believe that the power of so fixing our attention is the source of all that is highest and best in our intellectual self-education, as, in another direction, it is the source of all our moral self-improvement.

The automatist will say that your doing so is merely the result of the preponderance of one motive over the other,—the desire to go on with your study being stronger than the attractive or distracting influence. But if this be the whole account of the matter, why should we have to "make an effort,"—to struggle against that influence? We choose, as it seems to me, which is the thing that we deem preferable; and we then throw the force of the Ego into the doing of it, just like a man who makes a powerful muscular exertion to free himself from some restraint. And I hold that just as the Ego can turn to his own account the automatic action of his nervo-muscular apparatus, regulating and directing his bodily movements, so he can turn to his own account the automatic activity of his cerebrum, regulating and directing the succession of his thoughts, the play of his emotions. That succession is in itself automatic; you cannot produce anything, otherwise than by utilising what may spontaneously present itself; and you do so by the *selective attention* of which I have spoken, intensifying your mental gaze so as to make the object before you call up some other, until you get what you are seeking for. This you may readily trace out for yourselves if you will observe your own mental experiences, in trying to recollect something. And what shews the essentially automatic action of the cerebral mechanism in this familiar operation, is that after you have been for some time trying in vain to recall some forgotten name or some recent occurrence which has "escaped your memory," it will often flash into your mind

some little time afterwards, when you have turned your attention to something else. In the same manner many important inventious and discoveries have proceeded from the automatic working of the Cerebrum, set going in the first place by the determinate fixation of the attention on the object to be attained; the success of the result being due to the whole previous "training" of the organ.

The act of fixing the attention, in my belief, lies at the foundation of all education, and is one to be fostered and encouraged in every child. It is better to begin with only a few minutes at a time; gradually, by encouragement, the child comes to feel that it has a power of its own to prolong its attention; and at last the encouragement is no longer needed, for the child that has been judiciously trained will exert all its determination to learn its lesson, in spite of temptations to go out and play or to amuse itself in any other mode. But if this determination were simply the expression of a preponderance of motive, I do not see why an *effort* should have to be made. If the motive to fix the attention be stronger than the attraction of any other object, or the prospective influence of the good to be gained be more powerful than the distracting influence, the mere preponderance of the one over the other would produce the result. But we know and feel that the making such a determinate effort, involves more expenditure, "takes more out of you," than the continuous sustained attention when there is no distracting influence; therefore, I say there is something here beyond the automatic preponderance of motive—the mark and measure of the independent exertion of the will.

Now this power, call it what we may, is capable of being strengthened by exercise—no power more so; neglected children being generally most deficient in it, and most carried away by their own impulses. No doubt a greater power of concentration is natural to some, and a greater mobility to others. But still I believe there is no healthy mind in which this power is not capable of being developed by training, just like the power of the limbs in walking. Its possession is the foundation of all intellectual discipline; without it we can do nothing good in intellectual study.

Look, now, at the moral side, and see how it operates

there. We begin by saying, "I ought not" to do so and so,—assuming a moral standard. Take the case, which is unfortunately so common a one, of a man who has a strong temptation to alcoholic indulgence. He knows perfectly well that an habitual yielding to that temptation will be his ruin. I have heard of a man who said that if a glass of spirits was put before him, and he knew that the pit of hell was yawning between, he *must* take it. This is an instance of the overpowering attraction it has for some individuals; but this generally results from habit; and it is over *the formation of habits* that the will can exert its greatest power, by fixing the attention on one set of motives to the exclusion of other motives. I do not say that a man can *bring* motives before his mind. He cannot do that—we can only take what comes into our minds; but he can direct his thoughts in a certain line, as it were, so as to find them. He can think of his family or the future, and so exclusively fix his attention on the consequences, as to withdraw it from the immediate attraction. That I take to be the best mode. A struggle goes on in the mind of many a man subject to temptation; but if he has strength of principle enough to resist the immediate tendency to wrong action, and so gets time to deliberate, he may thus nerve himself for the conflict. Many good resolutions are formed—we know what place is said to be paved with them—and we hope to realise them. We determine in ourselves that we will avoid particular indulgences. We may have some strong disposition to apply our powers to ill uses, to play some mean trick, or something of that kind. Most of us have temptations of self-interest—not less strong because not pecuniary,—as to gain credit that does not belong to us, and so on. We hold back—"pull ourselves together" is the phrase of the present time—and summon all our resolution and determination not to yield. There is something more, here, than mere preponderance of motive; for we determinately direct our attention to the reasons why we should or should not do the particular act. I believe that in such cases the mind is best withdrawn from the temptation, by *fixing the attention upon something else.* That is the real secret of victory. By fixing our mind upon the object, and saying "I won't do it," the temptation still

keeps haunting us. I have known many a struggle of this kind relieved by the determination to follow an entirely different course. We know that in cases of insanity, where a man is led by physical disorder to take a miserable view of everything relating to himself, the medical man sends him abroad, where he is attracted by a new set of objects —something which prevents his mind from brooding over his gloomy thoughts; and in that way, as his physical health improves, the man comes to feel that he can voluntarily transfer his attention from them to objects of interest round him. This, I believe, is the manner in which we should distract our minds from anything we feel and know to be unworthy of our attention;—we should find out something more worthy, and pursue it with determination.

I ask you to take as your guiding star, as it were, in the conduct of your lives, these four words—"I am," "I ought," "I can," "I will."—"I am" is the expression of reflection and self-consciousness, the looking-in upon our own trains of thought. If we do not feel "*I am*," we do not think of ourselves and our own nature—we surrender ourselves. "*I ought*"—expresses the sense of moral obligation. By steadily fixing our attention on the "I ought," the course of action is first directed right, and its continuance in that path becomes habitual. "Turn to the right and keep straight on," and you will find the doing so easy in proportion. Every right act, every struggle of the will against wrong, is the exercise of a power which strengthens with use, and will make the next act easier to you. On the other hand, every time you surrender your will to the temptations of self-interest, or sensual gratification, or anything that turns you from the straight path, there is a loss of power which makes the next effort more difficult. Then, "*I can*"—the consciousness of power, is the foundation of all effort. And, lastly, it is not enough to say, "I ought to do it, and I can do it," but we must *will* to do it. The "I AM," "I OUGHT," "I CAN," "I WILL," of the Ego, can train the mental as well as the bodily Automaton, and make it do anything it is capable of executing.

THE DAWN OF ANIMAL LIFE.

AM not quite sure that the title of the lecture may not some degree mislead you. It speaks of the dawn of life. ; the first glance you might suppose that I was going to ce you back into those mysterious ages, about which our cellent friend, Sir William Thomson, gave us a few hints his own at Edinburgh, when life in any form first made appearance upon earth. But that is not the subject ich we are going to study; though were we to do so, vill venture to say that it would yield to no other in portance and interest.

want rather to lead you to that particular part of our uiry which has reference to objects that we see living ler our own eyes, whose life-history we can trace, and which are entirely different from the things with which are more usually familiar; they constitute the one of life of which man, the summit of all organisation, stitutes the opposite and highest pole.

ou cannot have paid any attention to your own vidual condition without feeling alive to the fact your physical frame is an exceedingly complicated You know you have a number of organs, and a ber of functions are performed by these organs. You , in fact, a separate organ for every function, and y separate organ has a function of its own, that ot be interchanged with the function of any other 1. In social life you may get your friend to do work for a day or a week; but when you ask the o do the work of the mouth, the tongue the work

of the nose, or the nose the work of the ear, you know that such an interchange is impossible.

But when we go to the opposite pole of animal life, what do we find? Have we there the same complex structure, or the same division of duty? Do we find separate parts, which can be distinguished under the microscope, and in which we can trace this corresponding division of functions? I propose to give you the answer to this question to-night; and I think I shall be able to shew you, before we have finished our evening's work, that the lowest animals are the very opposite of man in the points to which I have referred; that there is none of this difference of organisation—none of this speciality of parts—none of this isolation—this separation of organs for the fulfilment of particular work; but that every part of the animal can do equally well what any other part of the animal can do.

We scientific men are frequently accused of using hard words. I venture to admit the charge, and justify it in a very simple and intelligible way, by proving that there is another black as black as our own. I will ask you to take up any ladies' crotchet-book, and if Sir William Thomson or any of our learned friends on the platform behind me can tell you, unless they receive special feminine instruction, the meaning of the marvellous symbols in the book, I will give them credit for possessing even more genius than I already do. What do I mean by this? I mean there are certain special ideas which have to be represented by certain figures, letters, or words; these symbols, when employed in the crotchet-book, are designed to shew you to lift up your needle here and put it through there. Technical movements must be performed in order to produce the wonderful crotchet patterns, and there are particular symbols made to represent these different movements. A lady learns to understand the symbols the moment she sees them, and conceives it natural anybody else should do the same; but when she comes to botanical or zoological terms, she thinks there is something wrong in the invention of all the hard names, though they are as necessary to science as her own technical terms are to needlework.

But I promise you to use as few such as possible, and this I shall be better able to do because the objects about which I have to speak are limited in number.

Let me tell you in the first place what you must do. Go to some water-tub that may be standing near your house, scoop up a little fluid from the inner surface of the cask, and in all probability you will find that the water there contains more minute jelly-like objects than elsewhere. Or take a similar drop of water from an old neglected flower-pot that has been standing in the rain for weeks. If these fail, get a drop of water from the bottom of some pond. If you examine it under the microscope, you are sure to find in it some little spots, as if drops of diluted gum had been introduced into the water. What are these gum-like substances? I will tell you, and in doing so use the first of my big words. Fortunately this word is one you all know something about. It is one that some five or six years ago almost seemed to frighten England from its propriety. I mean the word protoplasm. You all, I daresay, remember my friend Professor Huxley's celebrated Essay on Protoplasm. Now, I am going to talk to you to-night almost exclusively about this Protoplasm in one form or another. This same little drop of jelly is neither more nor less than a speck of protoplasm. But what is protoplasm? The very name signifies that it is the primary raw material out of which other things are organised. You have blood circulating in your veins. If you examine a drop of it under the microscope you will find it filled with little red granules; red corpuscles, as they are called. Each one of these is essentially a little granule of protoplasm; if you examine that blood a little further, you find moving amongst these red particles a few white and somewhat larger ones. Note these latter particles well, and you will see that they are very similar things to my little drops of gum or jelly that you find in the water. Not only so, but you will see that these two things agree in one important point—viz., they frequently change their shape. Now they are round, now oval, and indeed now pushing out little projections from their margins. These atoms of protoplasm in the blood differ extremely little in their essential features from the

simplest form of animal life with which we are acquainted. What is the condition of this earliest form?

In fig. 1 you have one of the smallest forms of this little animalcule, called the Protamœba, the first and earliest form of the Amœba, which latter, in plain English, is called the Proteus animalcule, because, like the mythic Proteus of old, it is perpetually changing its shape. Like the white globule in our blood, it is round now, oval five minutes hence, and in ten minutes more it may become altogether different.

What does he do when about to feed? There is near him a little tempting morsel, and he contrives to come into contact with it. He does not bite it, because he has no teeth. He does not take it into his mouth, for he has no mouth. He does the next best thing. He just edges himself up to it, and having done that, he contrives to imbed the morsel in his body. There is no aperture into his body, and whichever side he brings into contact with the food is equally efficient. He contrives to bury the particle in his substance, and so long as it is there he is extracting some amount of nourishment from it, after which he ejects it. It is a simple sort of proceeding, but efficient for the accomplishment of its end.

How does he multiply himself? By a process equally simple—viz., he splits himself in two. A simpler process than that you cannot find. When you were children, and wanted to share your cake with a younger brother, you divided it in two; but mark the difference between the Protamœba and the cake. When you divided the cake, you had only the two halves left. There was no increase of growth in each half of the cake to reward your virtue. But when the Protamœbæ have divided, simple as they are in structure, they contrive to extract some nutriment out of the water, so that in a short time each of the two halves becomes as big as the original creature was. This little act, performed by the simplest and most lowly of all animal organisations, shews that in them there resides that all-important power which we call vitality, and the possession of which, as I think, I shall be able to prove to you before I have done, distinguishes the organic from the inorganic world, and demonstrates the *existence* of life.

Let me now take you a step farther. Fig. 2 is a little fellow similar to the one already described. Observe that he differs in having a little speck, *a*, in his interior which looks as if it meant little, but it means a great deal. We have here made an advance in the complexness of organisation. We have put our foot on the first rung of the

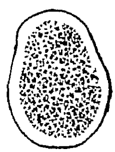


Fig. 1.
Protamœba.

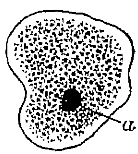


Fig. 2.
a. Nucleus.

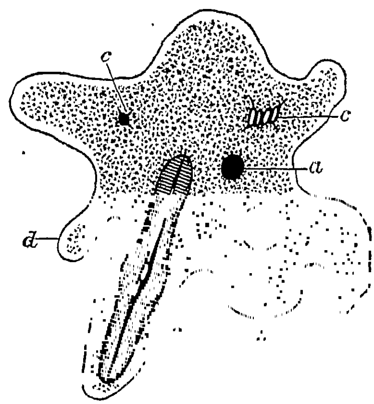


Fig. 3.—Amœba.
a. Nucleus. *b*. Contractile Vesicle. *c*. Vegetable objects upon which the Animal is Feeding. *d*. Pseudopodian Extensions of the Protoplasm.

ladder which will gradually lead us up to that summit which man occupies. Here is an organ of which we do not know the exact nature, but which we trace in an infinite variety of shapes both in animal and vegetable life. It has important functions of some kind or other to perform, otherwise it would not be so widely diffused. This little object is called the nucleus.

Let us now advance from the Protamœba to the Amœba itself. Fig. 3 represents one of the varied forms that it assumes. We have here, at *a*, a nucleus, like that in fig. 2. We find also at *b* a little transparent spot, and as that is always present we may be sure we have here an additional

organ. This transparent spot expands for a few minutes, is filled with a colourless fluid, and then probably bursts; at all events it disappears. It is a Contractile Vesicle; but what its function is we do not know. We do know that when it has contracted in the way I have described, it re-appears in the same spot, and in this way goes on expanding and contracting almost as regularly as your heart contracts and expands, but with very much longer intervals. Possibly it concentrates some nutritive essence, and every time it contracts or bursts, diffuses this essence through the system. I will not vouch for the correctness of this explanation, but here is undoubtedly a second organ, indicating an important advance in the complexness of the organisation. So far as nourishment is concerned, we find no change. The letters *c, c, c,* indicate objects which we know to be plants, upon which the creature is feeding. It has buried them in its substance, and is extracting nourishment out of them.

Thus we see that, though we have made this amount of progress in the development of the organism, we have not really attained to anything materially new in the physical or physiological history of the animal. It multiplies in the same way as the Protamœba, dividing into two; or sometimes poking out a little bit of an arm, as at *d,* it pinches off a bit of its tip, which floats away, and starts life as a new independent creature. At the same time, notwithstanding these successive subdivisions, the creature itself grows, maintaining its original size, and undergoes its usual variations of form.

Such is the history of the Amœba or Proteus animalcule—a history simple, it is true, but which gives us a clue to a very large number of other histories very much more complicated. At the first glance you may be disposed to say, What is the use of studying a little object like this? The same question might be applied to the minute forms of vegetable life, upon which I have no time to enter. If we had time to study all these minute plants, it would not be unprofitably spent, as they exhibit similar phenomena, demonstrating how universal are the laws upon which nature is built up, and by which she acts.

Such studies *are* of real use, because they throw a light even upon the constitution of man.

We must now turn to one of the most mysterious objects with which I am acquainted; one to which our attention was originally directed—as our attention is frequently directed—by the friend, whose name I was delighted to hear you cheer so heartily a few moments ago—I mean Professor Huxley. Examining some of the sediments brought up from the deep sea, and with which I shall have something to do in a few minutes, he discovered that the mud so brought up had a slimy character about it. He found that when portions of it were put into water in a fresh state, a sticky substance diffused itself in the water but with a definite outline, just as if you had dropped thick gum into the water, and the two refused to mix. He noticed the fact that imbedded in this gum-like substance there were numerous minute, organised points, which he called Coccoliths. He came to the conclusion, which I have no doubt was strictly correct, that this gum-like material, to which he gave the name of Bathybius, was an animal substance very similar to that of the Amœba, or Proteus animalcule, but with this difference, that whilst the Proteus animal was capable of being put under the microscope, being an almost invisible speck, the Bathybius substance extended for hundreds and perhaps for thousands of miles along the sea-bottom. Wherever certain materials formed the sea-bed, there you had this Bathybius. Whether we may speak of it as one animal, or an almost worldwide aggregation of minute animal points, I cannot say; but I agree with Professor Huxley in regarding this Bathybius as a condition of animal life in its very lowest form.

Dr. Dawson, of the Macgill College, Montreal, Dr. Carpenter, and two or three others, have investigated the earliest form of animal life yet found in a fossil state; this peculiar structure, called the Eozoon, has built up calcareous masses at the bottom of the sea on a very gigantic scale. These masses seemed to the above observers—and I think they are correct—to be the products of an animal having had a very wide diffusion, the nature of which has been something like that of the Amœba. Now, it gives

a strong probability to these views, that we have at the bottom of the Atlantic and Indian Oceans, and in various parts of the Pacific, at the present day, also on a very gigantic scale, a similar animal substance to that which Dr. Carpenter and others believe to have existed in ages gone by, and which constructed the calcareous Eozoon.

I will next direct your attention to some other curious forms. If you take up a little sand from favourable localities on the sea-shore, you will find that it frequently contains large numbers of exquisite, minute shells. Hooke, a celebrated microscopist in the days of Charles II., noticed the existence of these shells in sea-sand. From that time to the present, they have, at intervals, been made the objects of special study. They have at different times been put into all sorts of classes, and no wonder, since being so exquisitely beautiful and symmetrical, it is difficult to suppose they ever could have been formed by animals so low in the scale of organisation as the creatures that really did form them. They are now known by the name of Foraminifera, and the majority of them are so small as to be like dust. Unless the vision is aided by the microscope or the magnifying glass, you would not suppose them to be organised objects. They are so exquisite in texture and outline, and so variable in form, that Nasmyth, the engineer, always insisted that one little group which I keep in a microscopic slide, should be called "The School of Design." He said it contained more ideas for patterns and designs than he ever saw within so small an area. In most of these shells there are numerous chambers. Sometimes, as in fig. 4, these chambers are arranged in a straight line; in other cases they zigzag backwards and forwards, right and left alternately, as in fig. 5; and in others again they are spiral, as in figs. 6 and 7. These chambers represent so many successive growths. The shells, in many instances, are

Fig. 4.
Nodosaria.

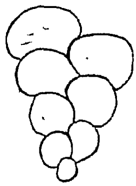


Fig. 5.
Textillaria.

perforated with numerous holes, as in fig. 6. The name of Foraminifera has been given to these objects because of these little holes. What do these perforations mean? I will tell you. The shells are made up of lime, which the creatures obtain from the sea in which they live. The

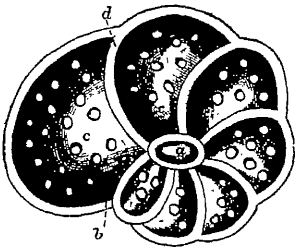


Fig. 6.—Young shell of *Discorbina Turbo*, shewing the foraminated walls of the chambers.

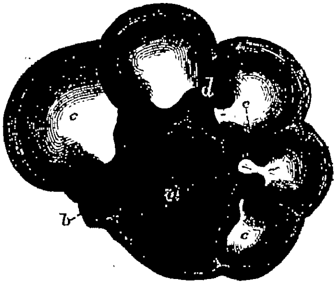


Fig. 7.—Young shell of *Peneroplis planatus*, with a non-foraminated shell. *a*. Primary segment. *b*. Aperture. *c*. Series of segments. *d*. Septa separating the successively added segments. *e*. Canals connecting the different segments, and once forming the orifice, like *b*, when each segment was in turn the outermost one. The same letters of reference apply to Fig. 6.

animals that tenant these shells are objects very like the Proteus animal. Fig. 8 represents one of these creatures with the animal inside. Notice streaming from its shell numerous delicate threads, which often blend together. Now, these threads are prolongations of the animal protoplasm exactly similar to those of the Amœba. They are much finer and more delicate, but in other respects are the same thing. These threads are called Pseudopodia, which means false feet; but they do more than act as feet, for they evidently collect from the sea the nourishment upon which the creatures subsist. When the shell has become too small to hold the growing protoplasm, a new joint or segment is formed at the end of those already

existing. The new ones grow out of the old ones like buds. Their arrangement is endlessly diversified.

The point that I next wish particularly to impress upon you is, that these shells have played, and are still playing a very important part in the physical history of the globe. It was remarked by Dr. Buckland thirty years ago, that these microscopic creatures, or similar ones, have played a far more important part than elephants, lions, or tigers, in the history of the globe. I will now prove to you that they have done so.

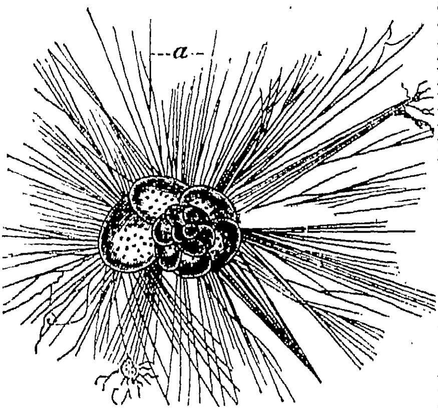


Fig. 8.—The same shell as fig. 6 with the soft animal projecting its numerous pseudopodia, *a*, through the foramina in the shell-wall.

Fig. 9 represents a round Foraminifer, consisting of a single joint with a little aperture serving as a mouth. This is the Orbulina. We find it in mud dredged up from our own North Sea,

Fig. 9.—Orbulina in its common condition. *a*. Oral aperture.

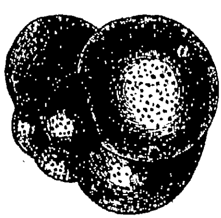


Fig. 10.—Upper surface of shell of *Globigerina bulloides* as usually found. *a*. The newest segment.

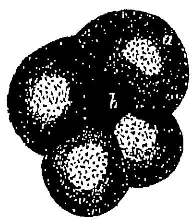


Fig. 11.—Under surface of fig. 10. *a*. The newest segment. *b*. The oral aperture.

between the Scotch coast and the Shetland isles. It early became plain to me that this little animal was a deep-sea form. I always found it associated with the shell, figs. 10 and 11, and which is so minute that it looks, when you view a lot of it with the naked eye, like the finest dust. A breath will suffice to blow it out of the box in which I keep it. This latter shell is the little object which has been the chief agent in modifying the crust of the globe.

When Ehrenberg, the great Prussian microscopist, was pursuing his investigations, and shewing the power of his genius by working with tools for which you would not give twenty shillings (it was not the tools, but the eye he contrived to put behind them that did the work) amongst other things he put under the microscope a little bit of powdered chalk. He found, to his perfect astonishment, and the astonishment of all of us, that the white Chalk, which runs in an almost unbroken line of elevated Downs from Flamborough Head to Dover and Beachy-head, and other points on the south-east coast of England, and which is generally about 500 feet in thickness—is neither more nor less than a vast accumulation of these minute shells. Not only so, but many of the shells of which that chalk consists are either the identical species represented by figs. 10, 11, or a variety of it, very slightly modified. The forms I am now describing to you are found at various localities, from the North Pole to the South Pole, and from the Red Sea to the middle of the Pacific Ocean. These shells underlie the deep sea, not universally, but in detached masses of vast extent. It is difficult to realise that the mass of chalk which, after having undergone a variety of chemical changes and been subjected to immense pressure, reducing its volume, could ever have been produced by such minute agents.

When the bed of the Atlantic was surveyed, preparatory to carrying out those great works with which your distinguished townsman, Sir William Thomson, was so intimately associated, it was found that a great part of the bed upon which the electric wire was to be laid consisted of these shells. We know now that a great part of the sea bottom between us and America consists of such shells, with a

few minute, siliceous objects mixed with them, and whic combination in all probability constitutes a mass hundred of feet in thickness. We have no means of ascertainin its exact depth, but it is permeated through and throug with Huxley's Bathybius. It is in virtue of this peculia foraminiferous accumulation, forming a soft bed for th reception of the cable, that the success of Anglo-America telegraphy is largely due.

We have recently obtained some very important infor mation about these little objects from the "Challenger." A variety of investigations have been carried on, and differen conclusions arrived at by different individuals, as to where these shells live. Some, like Mr. Gwyn Jeffreys, insisted that they live near the surface of the water, and that the deposits are merely accumulations of dead shells which have sunk to the bottom. Others contend that they live at the sea bottom and die there—that being their home But the very last information we have had from the "Challenger" has thrown a wonderful light, not only upon the position in which at least some of these creatures live, but upon the extraordinary appearance they present when living.

It appears that when these creatures are living at the surface, both the Globigerina and the Orbulina are armed with innumerable delicate, flexible, calcareous spines, as represented in figs. 12, 13. When the animals perish these spines fall off, and the shells descend to the bottom of the ocean. Such spiny investments probably characterise the younger states of the animal.

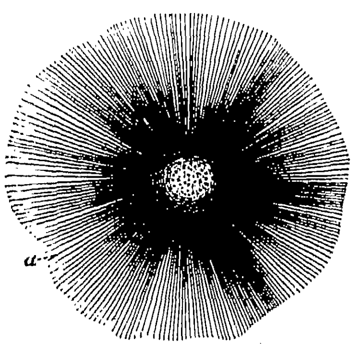


Fig. 12.—Orbulina as living at the surface of the sea. *a.* Calcareous spines projecting from the shell.

We advance now to another group of objects. When, in the days of

Hooke, of whom I have already spoken, attention began to be directed to the microscope, it was found that if a little vegetable matter was allowed to stand for a few days in water—in other words, to form what the druggists call an infusion—minute living objects appeared in it. Finding these objects in such infusions, and not being aware that they were to be found elsewhere, naturalists gave them the name of Infusoria. These became the special study of Ehrenberg; but he mixed up with this common group an enormous number of objects that were really plants. He was a magnificent observer, and had a wonderful genius for classification; but, unfortunately, he was not a physiologist; consequently he gave to these vegetable forms eyes, stomachs, teeth, and a whole host of other organs, which it is not usual for plants to have. About one-half of the things called Infusoria proved to be animals, but the other half turned out to be plants. His labour, however, was not thrown away. Investigations which were pursued into the history of these creatures demonstrated that they were closely allied to the Amœba, but with a decided advance in organisation. The animal consists of a mass of protoplasm, but in the interior we have a nucleus (figs. 14, *a*, to 16, *a*), a contracting vesicle (figs. 14, *b*, to 16, *b*); in addition to these organs we find, in the first place, that each animalcule is clothed with an external skin, which the

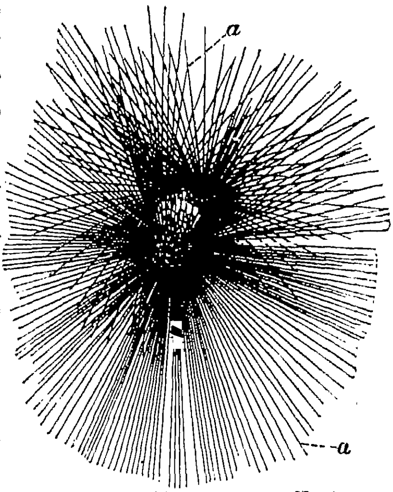
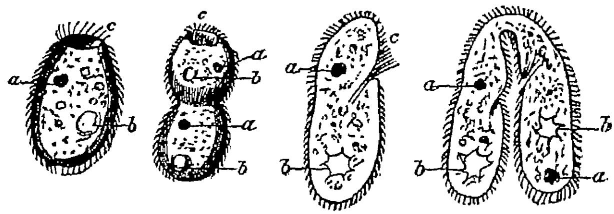


Fig. 13.—Globigerina. *a*. Clusters of spines projecting from each segment of the shell.

Amœba does not usually possess. Not only so, but w find that, in one part or another, there is a mouth—that is, an opening—where the skin is inverted into th interior of the animal for a very short distance like a tube and that this is the inlet through which the food enters



Figs. 14 and 14*.—A flask-shaped infusorial animal. *a.* Nucleus. *b.* Contractile vesicle. 14* is preparing to undergo fission transversely.

Figs. 15 and 16.—Paramecium. *a.* Nucleus. *b.* Contractile vesicle. *c.* Mouth. Fig. 16. is undergoing fission longitudinally.

The skin is furnished with innumerable minute moving threads, called cilia, resembling the vertical threads forming the *pile* of velvet, which are everlastingly at work, yet what moves them we do not know. But these ciliary movements acting in a variety of ways enable the animal to swim, which is their ordinary function.

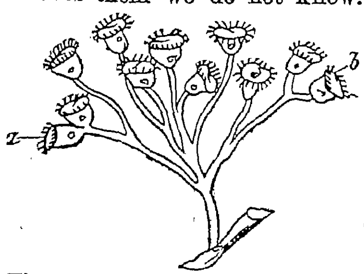


Fig. 17.—Carchesium. A flexible branched infusorian animal. *a.* An animalcule beginning to subdivide. *b.* Another, in which subdivision has reached the flexible peduncle.

Fig. 17 represents a fixed cluster of similar objects growing like a tree; a condition produced by a process of fission which allies these creatures to the Amœba. They multiply by splitting themselves into two, the process being effected in varying directions, and with different degrees of completeness. Sometimes the division will be lengthways, as the animal represented

in fig. 15 is separating into two lateral halves in fig. 16; but sometimes they take a fancy to split in the opposite direction, as fig. 14 is doing in fig. 14*. In the Infusoria generally this splitting is done effectually; but fig. 17 is a creature which has done the splitting ineffectually. One individual of this cluster originally attached itself to a fixed object, and then elongated the footstalk by which it so attached itself. It then split into two, as just described, the divisions extending into the footstalk, but not reaching its base. Similar processes were repeated again and again, as at *a* and *b*, until the organism attained to the tree-like form represented in fig. 17.

Here you have something like what I daresay you people in Glasgow understand very well, viz., practical co-operation. To a certain extent independent, each animalcule obtains its own food, but they are all acting in harmony, nourishing the common structure of which they form mutually dependent parts. I take it something like that is the true mission of human society. No one can do without the other, any more than these animals are independent of each other. In social life we recognise that the rich are necessary to the poor, and the poor are necessary to the rich. Each have their own appropriate work to do in the world. The young require the experience of the old, and the old frequently require the energy and active force of the young. All are mutually dependent upon each other; and though it is perfectly true of any one, that society can do without us, yet, viewed as a whole, we are mutually dependent one upon another.

I will now call your attention to some objects of which the history has been worked out by two gentlemen at Liverpool —Dr. Drysdale and the Rev. Mr. Dallinger. They have devoted their energies to the investigation of a very minute species of Infusoria, fig. 18, which was furnished with two large and very peculiar cilia, *b*, at one end. They noticed that when

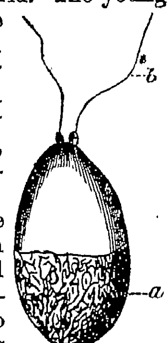
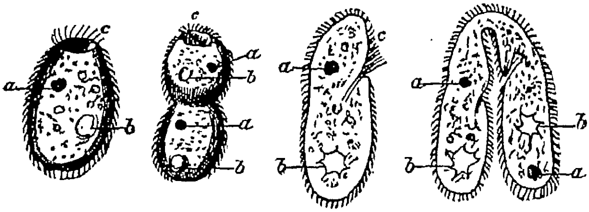


Fig. 18.—Cercomonad. *a*. Granular portion. *b*. Cilia.

Amœba does not usually possess. Not only so, but we find that, in one part or another, there is a mouth—that is, an opening—where the skin is inverted into the interior of the animal for a very short distance like a tube, and that this is the inlet through which the food enters.



Figs. 14 and 14*.—A flask-shaped infusorial animal. *a*. Nucleus. *b*. Contractile vesicle. 14* is preparing to undergo fission transversely.

Figs. 15 and 16.—Paramecium. *a*. Nucleus. *b*. Contractile vesicle. *c*. Mouth. Fig. 16. is undergoing fission longitudinally.

The skin is furnished with innumerable minute moving threads, called cilia, resembling the vertical threads forming the *pile* of velvet, which are everlastingly at work, yet what moves them we do not know. But these ciliary movements acting in a variety of ways enable the animal to swim, which is their ordinary function.

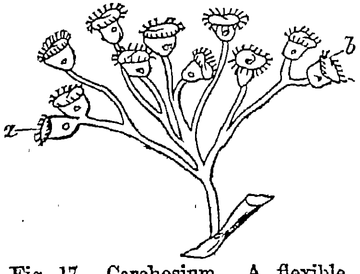


Fig. 17.—Carchesium. A flexible branched infusorian animal. *a*. An animalcule beginning to subdivide. *b*. Another, in which subdivision has reached the flexible peduncle.

Fig. 17 represents a fixed cluster of similar objects growing like a tree; a condition produced by a process of fission which allies these creatures to the Amœba. They multiply by splitting themselves into two, the process being effected in varying directions, and with different degrees of completeness. Sometimes the division will be lengthways, as the animal represented

or made of something very like flint. These siliceous skeletons when living are covered with a sarcode or protoplasmic substance. Figs. 19 and 20 represent two elaborate skeletons of these Polycystineæ deprived of their sarcode. They are objects belonging to the same group, and having the same low organisation as those already described.

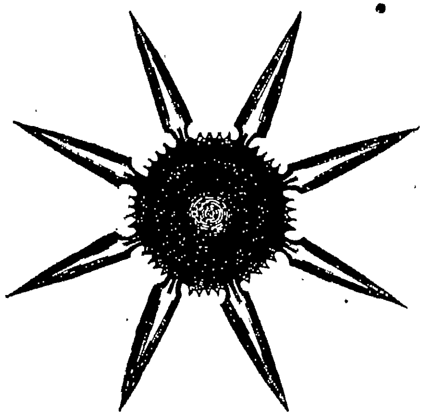


Fig. 20.
Siliceous skeleton of a Polycystinean.

We will pass from them to the Sponges. If time permitted I could shew you that the history of Sponges is quite as interesting as that of any of the other animals we are acquainted with. You are all familiar with the sponge of commerce—that elastic horny substance. When that Sponge was growing in the sea it was covered with slime. It is washed before it comes into your hands; if it were not, you would find it had a slimy, repulsive, nasty feeling, like that you experience when you lift up a snail. Now, this Sponge, with its horny skeleton, has a very curious history. If you notice closely you will find little punctures scattered all over its surface. When the sarcode or flesh invests it, these little pores absorb water. You will observe that at some points there are large apertures. Very generally these larger apertures form the summits of conical projections like the craters of miniature volcanoes. The water absorbed by the smaller openings passes out at the large ones.

Fig. 21 is a diagram, copied from one of Professor Huxley's, and which will illustrate what I mean. The black tint

represents the substance of the Sponge, the skeleton, and the sarcode or protoplasm included. The arrows indicate the direction in which the water flows through these canals, and then emerges through the larger volcano-like apertures (fig. 21, *a*). It has long been a perplexing question how the water is forced through, but it lately has been discovered how this is done. It is found that some of the canals expand into little chambers, *c*, which are lined with cilia, or little appendages like those which enable the Infusoria to move. These are in constant action. The interior of the

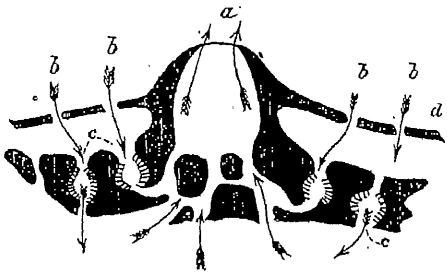


Fig. 21.—Diagrammatic representation of a section of a Sponge.
a. Exhalant aperture. *b b*. Inhalant apertures. *c*. Ciliated chambers. *d*. Surface tissue.

human mouth, as well as the interior of the bronchial tube and lungs, are lined with these cilia, which are constantly moving in such a peculiar way that the movement appears to be always in one direction. Their action resembles that of a field of corn when a breeze is blowing over it. We see wave after wave going from one side to the other, but we know very well that although the heads of corn appear to be travelling in that direction, the corn does not go, but that this appearance is caused by the sudden bending down of the corn in one direction, and the slower restoration of it to its old position. The quick movement strikes the eyes, but the slow movement is not noticed. Something of the same kind exists in these cilia. The consequence is that in the Sponge these little cilia keep up a movement in one direction, just as those in the interior of the lungs keep up a movement from within towards the

throat. But for this movement we should be in danger of choking. The action causes an outward flow of mucus, and brings it up through the windpipe towards the throat where, by a vigorous cough, we can throw it out. Just in the same way the cilia of the Sponge produce currents in the sea water, which is drawn into the Sponge at the points *b*, *b*, and is being expelled again at the larger aperture *a*.

I have now shewn you the life-history of the Sponge, so far as mere nutrition is concerned. But Sponges will die like other folks, and their place has to be refilled; before shewing you how this is done, let me describe some other features of their internal structure.

First, observe that, in addition to the horny substance that forms the skeleton of the Sponge, we have spicula—little spines. Sometimes they are like pins with heads to them, and sometimes like needles. Sometimes the spikes are adorned with all sorts of fringes, constituting some of the most beautiful objects that you can purchase from dealers in microscopic curiosities. These spicula form additions to the skeleton. We find in one group that they consist of carbonate of lime. In such instances they are triradiate, like the three legs of the coat-of-arms of the Isle of Man. When three points radiate from a common centre you may be sure they are calcareous, but when they are like needles you may generally conclude that they are siliceous.

One new form of Sponge has been brought to us within the last few years, the first specimen of which is now in the British Museum. For this specimen £30 was paid. Then a few more came to England, and they were sold at prices ranging from ten to fifteen guineas. One day a friend of mine connected with the custom-house in London, was at the custom-house, when another of the officers said to him, "Come here and I will shew you something that will astonish you." He pointed to one box in which there were hundreds of these costly objects, then to a second which contained a similar number. Nobody could find out whose they were, or to whom they were consigned; nor, up to the present day, has it been discovered, so far as I know, where they went to. Now and then an odd one

came out, and was sold for ten guineas; now, however, they are to be bought for a few shillings each. The Euplectella, as this Sponge is called, is the loveliest object ever produced either by nature or art, and in saying that I am aware I am saying a very strong thing. It is sometimes called the Venus's Flower-basket, and is composed of delicate threads of silica, like spun glass, arranged in regular geometric patterns. It is almost incredible that so exquisite a design could be produced by a mass of jelly-like substance similar to the Amœba, and which is not one bit more highly organised than I have shewn that animal to be.

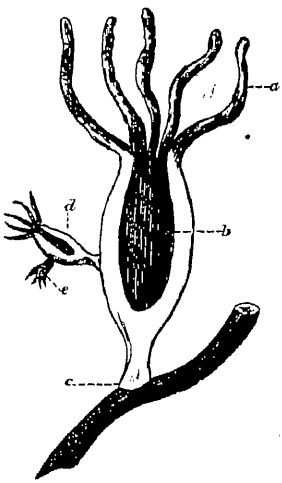


Fig. 22.—Diagrammatic representation of a section of a Hydra. *a.* Tentacles. *b.* Digestive cavity. *c.* Foot attached to a plant. *d.* A young offshoot. *e.* A similar offshoot of the third generation.

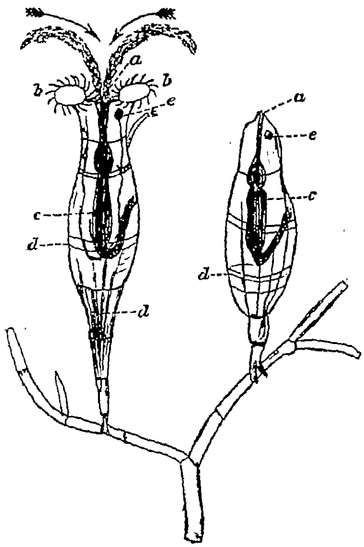
What gives that mass of jelly the power of constructing these elaborate skeletons, arranged in such geometric forms? It is the wonderful force which we recognise under the name of *Life.* When men tell me that by bringing together certain combinations of inorganic elements, they can produce gelatine and albumen, and various other animal substances with which we are familiar—seeking to make me believe they have taken the first step in producing life, I challenge them to produce anything like what I have just described. I do not deny they may succeed in getting something like albumen or gelatine, but it is essentially *dead*—it possesses none of the powers of living protoplasm.

We may next go to the Hydra, the fresh-water Polype found in the ponds of our own neighbourhood. There is a decided break between the animals I have just been describing and this Polype. Fig. 22 exhibits a much more elaborate

construction than we have hitherto seen. We find arms, called tentacles, at *a*, a true stomach or digestive cavity at *b*, and a foot at *c*, by which the object attaches itself to fixed bodies. Not only so, but there are sprouts growing out of its side which are chips of the old block. These offshoots grow precisely as the bud grows from the side of a young tree. You sow the seed of the oak, and know that in due season it will spring up as a single shoot with a couple of leaves. Watch its development, and you will see that leaf forms after leaf; by and by the stem begins to throw out buds from its side, forming a branching structure. These buds are produced in a perfectly intelligible way. If I had time, I could demonstrate that the process of their formation is very much the same as that by which the branch *d*, of the Hydra has been formed. The latter shoots out from the side of the parent animal like the vegetable bud; not only so; this second generation is often seen throwing out a third one, *e*. When the organism reaches this stage, the young growths usually become detached, and pursue life on their own account. By and by similar buds form both on them and on the parent animal, repeating the same history as before. These fresh-water Polypes exhibit a life-history which gives us the clue to that of numerous animals which we find on the sea shore. You are familiar with the corallines—the northern representatives of the corals found abundantly in tropical seas. These corallines are flexible, horny, and branch very elaborately. But when we examine their mode of growth we find it to be just like that of the Hydra, with this difference—in the Hydra, the young branches become detached from the parent stem; the coralline animals, being encased in a horny coat of mail, are unable to detach their young shoots; hence they go on branching until they develop into complicated structures, often sustaining thousands and tens of thousands of Polypes upon a single stem.

Associated in the same infusions with the Infusoria about which I have been speaking, we often find another extraordinary group of creatures. Figs. 23 and 24 represent the most common of these Rotifera, as they are termed, in two states. Fig. 24 exhibits him in

a somewhat quiet condition, anchored by his toes, as they are sometimes termed, to a twig of an aquatic plant. When he chooses he can extend his snout, *a*, and throw out two round wheels, as at fig. 23, *b*. When we watch the action of these wheels, it is difficult to resist the conclusion that they are spinning round and round. But they are not. Round the edge of each of these circular discs there is a circle of cilia. When these cilia are in extremely quick and active motion, they give to the two discs an appearance of revolving in opposite directions, whereas, in fact, they are merely creating currents in the water which move in the directions indicated by the two arrows.



Figs. 23 and 24.—Two animals of *Rotifer vulgaris* adherent to a plant. 23 has his ciliated discs, *b*, *b*, fully expanded and in action. *a*. Mouth. *c*. Stomach. *d*. Longitudinal and circular muscular bands. *e*. Brain. In fig. 24 the discs have been drawn back into the anterior part of the body.

These currents are really two whirlpools which meet in the middle, and convey such particles as the water contains straight to the animal's mouth. Thus his food is brought to his very door. All he has to do, when he sees a tempting morsel, is to make a snap at it, or if it is not tempting, he lets it go by.

When I hear of a man making £100,000 honestly, I consider there is usually something more in this than mere chance. I do not much believe in luck in this world. What is termed such, is often the union of a good head, self-

denial, and industry. It is the combination of these qualities that raises one man above another. The qualities may not be of the highest intellectual order, but they are qualities worth having, provided they are honestly used. Such a man sets *his* wheels to work, and produces currents which converge at one point—his pocket. If there are any of the class of operatives present, let me remind them that the study of nature ever teaches us the necessity for mutual dependence. Remember that whilst our wealthy friend is setting these streams in motion, and they are flowing legitimately to the fixed point I have indicated, every one of them is passing your mouths, and each operative who aids in producing these currents takes his snap at the good things which they convey. He gets his share; and if he is sober and diligent, and exhibits the same qualities as his master does, he will not only rise higher and higher in the social scale, but sooner or later may be a master himself, when we may hope the streams will meet in *his* pocket.

These Rotifera have a digestive system, *c*, longitudinal and circular muscular bands, *d*, and a true brain, *e*. The bodies of these animals are so transparent that we can see everything going on within them.

The study of these Rotifera is an extremely interesting one, since there are some curious points in their history. Dr. Carpenter tried an experiment, which has also been tried by others. He froze a number of these objects in a watch glass; on thawing them again he found that whilst some had perished several were still living. He then froze the living ones a second time, when a few more were killed. A third freezing destroyed them all. These Rotifera are common in the spouts and gutters on the tops of houses. Sometimes these gutters are filled with water, when the animals are lively; but in the heat of the summer season they are often dried up, when the Rotifera are reduced to particles of mere dust. Some time ago the Rev. Lord Sydney Godolphin Osborne, the well known S. G. O. of *The Times*, sent me some Rotiferous dust in a pill-box. He had had it in his possession for months; but when I put a little of it into water, in less than five minutes the animals which it contained were all in full action. I first saw little specs

of jelly, which in a few minutes expanded into perfect Rotifera. They had been in the dormant state in which I received them for months; but when they fairly got into the water they put their wheels to work, and looked as if they were uncommonly hungry. It was quite amusing to see how readily they took to their work of feeding. I kept some of the dry dust in my possession for seven or eight months, and at the end of that time, when a little drop of water was brought in contact with them, they were as active and vigorous as ever. Here are creatures of high organisation, yet so endowed with peculiar qualities, that whether, on the one hand, we freeze them, or on the other dry them up, they still live.

This subject of vital endurance leads us to the consideration of an allied, though different problem. You are all, probably, aware that within the last few years an old question has again come to the front and attracted the attention of both the wise and the unwise, the learned and the unlearned. I mean that of spontaneous generation.

Our forefathers two centuries ago knew little of the inner secrets of nature. They supposed that frogs and other highly organised creatures were brought into existence by spontaneous generation—meaning by that that they were formed in some mysterious way, out of dead, inorganic, slimy earth, without the previous intervention of living creatures. When investigation proved that these higher animals had not been formed in this way, men applied the same hypothesis to other creatures of a lower order. Throughout the history of this subject the advocates of this doctrine have always retreated from position to position, from the known to the unknown—in the same way that witches, races of pigmies, and anthropophagi with heads under their shoulders, were always described as existing in remote places about which little was known.

The progress of zoological knowledge having shewn that none of the higher creatures were produced by spontaneous generation, the Infusoria, of whose life-history very little was known until within the last twenty years, were confidently referred to as illustrations of the doctrine; but later

investigations have shewn that none of the higher forms of these minute creatures are exceptions to the general rule that life alone produces life; and consequently the supporters of the doctrine are now compelled to fall back upon the lower Infusorial types—creatures which are so minute that the study of their life-history becomes in the greatest degree difficult, and the results of that study obscure and doubtful. Let it be distinctly remembered that wherever we have been able clearly to ascertain how their reproduction was effected, it has proved to be in the strictest accordance with that of creatures of higher organisations than themselves.

Dr. Bastian, of London, the modern advocate of the disputed doctrine, makes certain infusions, and he finds that certain minute objects appear in them, which he declares to have been produced by spontaneous generation. He heated the fluids in which these things are developed, raising them to such a temperature as, in his opinion, must necessarily kill all the germs of life. The experiments, we are assured by him, were conducted under such precautions that no such germs could re-enter the infusions from the surrounding air. Yet these infusions still exhibited, in a short time, minute forms of animal and vegetable life. On the other hand, a very distinguished naturalist in Paris, M. Pasteur, declares that if such experiments are properly conducted so that the heat is equally diffused, every germ being actually killed, and no new germs admitted, this result will not be obtained. It is necessary for you to understand that the air is literally filled with such germs; you inhale them with every breath you take; whether or not they produce disease, as some suppose them to do, I will not venture to say.

In these cases the question resolves itself into one of accuracy of observation. My friends, Dr. Roberts, of Manchester, Professor Huxley, and others, have gone over the same ground as Dr. Bastian has done, and their conclusions are more in accordance with those of Pasteur than of Bastian. They say that when the observations are properly carried on, and care is taken to exclude life from the bottles, no germs re-appear.

I venture to affirm that in cases of this kind one positive

observation is worth a hundred negative ones. If Dr Bastian tells me he has taken all needful precautions, and yet he invariably gets these germs, I venture to ask whether it is not possible there has been some undetected loophole through which the germs may have entered the infusions. But, on the other hand, when I find that a man like Dr Roberts conducts experiments again and again, and obtains positive results, the opposite of those obtained by Dr. Bastian, and this too, after his infusions have been kept for months, I say, one such observation is worth a hundred negative ones.

Within the last few weeks some important observations, bearing upon this subject, have been made by the Rev. W. H. Dallinger and Dr. Drysdale. Owing to some remarkable peculiarities in the aspect of the minute Infusorian which was the subject of their study, they were enabled to follow its life-history with considerable success. This result was obtained through persevering observations, during the continuance of which the two observers successively relieved each other at the microscope. Fig. 18 represents a common form of the Monad, as the minute Infusorian in question is called. They found that it multiplied through various modes of mechanical subdivision, or fission, as in the animals described in an earlier part of this lecture; but the portion of its history which bears in the most important manner upon the question of spontaneous generation is connected with the peculiar structure of the posterior half of the body of each Monad. As shewn at fig. 18, *a*, this portion of the organism consists of a more granular form of protoplasm than its opposite extremity. Under special circumstances the skin enclosing the granular part becomes ruptured and the granules are liberated. The observers found that each of these liberated granules developed into a perfect Monad in the course of a few hours.

These facts shew us that the process of Fission occurs amongst these very minute forms, which are not more than $\frac{1}{1100}$ of an inch in length, just as in the Sponges, and in the larger Infusoria. Remember that we are now considering creatures so minute that fifty years ago it was almost

beyond the powers of existing microscopes to make any trustworthy study of them possible. The lens of the microscope used was 1·50th of an inch focal length, while few persons are in the habit of using lenses of higher power than the 1·8th or the 1·12th of an inch in searching out the hidden secrets of nature. Yet, on applying this power of 1·50th of an inch to these minute objects, we find the same laws of organisation and reproduction prevailing amongst them as exist amongst the more conspicuous of these early forms of animal life.

These gentlemen exposed the above animals on seven occasions to a dry temperature of 121° centigrade, which is considerably above the boiling point of water, and found that it killed all the parent and mature animals, but in two instances it did not kill the granular germs. They found the latter still living, and watched their development into the well-known mature forms. They thus obtained the proof, supposing that these experiments were accurate, of what we long before believed to be true, viz., that the temperature which killed the parents left the germs possessed of life.

That this was the true explanation was shewn to be probable by their next experiment. They raised the temperature *of the fluid* in which the animals lived to 66° C., and found that this sufficed to kill all the adults, whilst the germs survived even after that temperature had been raised to 127° C.

If I were asked to believe that a man walked through the streets of Glasgow with his head under his arm, I should obviously want a very strong amount of evidence before I believed the statement, and so would you. So it is with any alleged facts that run counter to the known history of protoplasm; we demand unusually strong evidence before we accept such allegations. We have seen what protoplasm does on a large scale in the larger animals, and as we trace it downwards amongst the minuter forms of life, we see that in its nourishment, its growth, and its multiplication it exhibits certain phenomena that have a common existence in all organisms from man down to these lowest Monads. The little atoms of protoplasm contained in the human

blood multiply by subdivision in the same way as the protoplasm does amongst these creatures. The Protoplasms which fill the cells of all animal bodies, as well as those universally diffused throughout the vegetation of the world, are multiplied in the same way by successive fissions as in those I have described to you. We thus see that when we trace the highest vegetable organisms down to their simplest states, we find precisely similar phenomena to be of universal occurrence.

Hence we have every reason to suppose, judging from analogy, that if we succeed in effectively studying forms yet more minute than those with whose life-history we are already acquainted, we shall find the same processes still in action. When Newton and the older astronomers turned their telescopes to the stars, they knew nothing of the remote systems which modern astronomers have succeeded in discovering. Newton shewed us how the nearer planets and the remoter stars obeyed the same law of gravitation. Have our modern astronomers, as they discovered yet remoter planets and newer nebulæ, found the law of gravitation reversed? No; they see the same mysterious force regulating the movements of the most distant as well as the nearest suns. They find everywhere in the heavens unity in the forces of nature; in like manner when we have traced these vital processes down amongst these minute atoms, and found no material change in their nature, we are fairly justified in assuming that as we trace them yet lower, we shall discover a similar continuity of known vital actions. If this be true, we are landed in a position which makes me utterly fearless in relation to the investigations of science. I know I have nothing to do here with questions of theology. But in admitting the possible correctness of some scientific doctrines which to you may appear to have dangerous tendencies, do not imagine from my making such admissions that I am *a priori* hostile to that Christian faith which I cherish in common with most of yourselves. It may be true that my remote ancestor was a monkey. I do not care if he was; he has done me no harm, and may have handed down to me some vigorous activity of body, the result of his woodland life. Neither do I care if evolution

proves to be true—if every living organic thing that now appears in nature once emanated from one solitary germ of life. I do not fear, because, instead of such a theory enabling us to dispense with the God of nature, the recognition of a creative Power is as necessary to explain the origin of that one primary germ, as to explain the existence of the world itself. If it be true, as I have been teaching you, that no life appears, except as the product of pre-existing life, it is equally true that there has been an unbroken continuity of life from the beginning down to the present time.

I believe that the philosophy of the evolutionists embodies a vast amount of truth, and that their inevitable recognition of the fact that organic life had a beginning upon the earth, and had not existed throughout eternity, is not the least important of the conclusions involved in that philosophy. The high-priests of the evolutionist school tell us that organic life is now a much more complex thing than it once was, and that the more complicated organisms have been gradually evolved out of others that were less complex. We are thus led back to a point of time when the first germ of life dawned upon the earth; this is important, because we have not the same proofs that the *inorganic* universe had such a beginning. Those who argue in favour of the eternity of matter are not so easily answered as are those who contend for the eternity of life and organisation. But if there was a time when so important an element of the world's being as life was non-existent, and yet another time when it began to exist, it becomes somewhat probable that the inorganic world had a beginning likewise. Be that as it may, the truth of which I am speaking accords, as far as it goes, with the first of our great scriptural beliefs—viz., that there was an origin and a Creator of all things.

The advoc[illegible] of spontaneous generation may however admit this [illegible]. They may contend that they only connect creation with [illegible] Creator at an earlier stage of His work, and that He who endowed the first germ with its marvellous functions displayed a yet mightier power when He endowed the dust of the earth with similar poten-

tialities. This is doubtless true; but I contend that we yet lack all proof that dead, inorganic matter can be converted into living matter, save through the agency of pre-existing life; and it is my firm conviction that no such proof will ever be obtained. The chemist may so combine atoms as to obtain products like *dead* albumen and similar animal substances, but he has hitherto failed to endow them with life. They cannot imbibe nourishment from without, multiply by dividing and subdividing, ever growing as they do so; in a word, they lack *vitality*.

If any who now listen to me think that I am lowering their grand conception of a Deity by thus possibly reducing part of His creative work to the production of a solitary germ, I confess I cannot agree with them. If such a germ contained within itself the potentiality of development into the entire living creation, I confess I cannot conceive of a higher manifestation of creative power than is involved in its production. The being who originated such a germ must at least correspond with our feeble conceptions of Him whom we reverently call God. If so, I think we cannot avoid going a step further, and exclaiming with one of England's noblest intellects—

> "If there's a Power above us,
> And that there is, all nature cries aloud
> Through all her works, He must delight
> In virtue."

COAL AND COAL PLANTS.

Professor Williamson said—On walking to-day through one of your most busy thoroughfares, as I met face after face in the moving crowd, I could not help thinking how intense would be the interest if I could have placed in my hands an accurate and minute history of the life, the sorrows and the joys, the difficulties and the triumphs of each one of the individuals that I thus met. If such a record of even the humblest of those whom I saw to-day was placed in our hands—one that revealed the whole innermost heart and soul of the individual—I need scarcely say how great would be the interest of such a story. In precisely the same way, and in accordance with the same principles, I hesitate not to affirm that there is no object in Nature of which the Almighty is the author, that, if expounded by those who know all that can be known respecting it, is not capable of exhibiting a similar measure of interest. Every object in creation has had a beginning and a progressive life—has undergone a series of changes, the results sometimes of the operation of dead forces and sometimes of living ones; we never see anything that has always been exactly what it is when our eye rests upon it; it has in every instance once been something different to what it is now; and the attempt to follow the changes it has undergone, and the causes that have produced those changes, cannot fail to produce a measure of interest in the mind of every intelligent being. In accordance with the wish of your Committee, it is my object to-night to deal, so far as my humble powers will enable me to do, with Coal. Coal, as you know, is in itself a sufficiently dark object. But as the illumination of this room shews, when properly treated it is capable of giving out a considerable amount

of physical light. But instead of looking at it to-night in reference to the light it can thus throw upon us, it is my object to see what amount of light I can succeed in throwing upon it.

The first question that meets us in a study of this kind is, What IS coal? Having obtained the answer to that query, we will next try to learn what coal WAS; that point also being settled, we will endeavour to ascertain how it has been changed from what it was to what it is, and how it came into the places where we now find it. At a very early period in the study of geological science the question of the history of coal became a prominent one. At first it was supposed, as you will believe probable, that coal was a mere mineral product, as gold and iron and other similar things are mineral products. But at length the probability that it might have had a vegetable origin dawned upon the minds of some thoughtful men. This doctrine of the vegetable origin of coal soon received universal acceptance from the scientific public. But as there may be some here who have not had an opportunity of tracing the process by which scientific men have arrived at this conclusion, let me try to make its more prominent points plain to you.

Fig. 1.—Pinnule of a Fern-frond from the Coal measures.

Thanks to the assistance of my friend on the platform, Mr. James Thomson, who has exerted himself on your behalf in bringing together the specimens now before me, I have in my hand a piece of blue shale obtained from a colliery. It is merely a piece of hardened clay, but upon the surface of this shale I find impressed, in distinct and unmistakable shape, the representation of a fragment of a fern (Fig. 1). There is and can be no doubt whatever that this is not

something presenting a mere resemblance to a fern leaf, but that it was once part of a living plant. On examining this specimen further, I see that it is no longer green but black. I scrape a little of this black substance from off the stone, I subject it to the ordinary chemical tests, and I find that in every point it proves itself to be coal. I thus see that, however the process may have been accomplished, there is in Nature a power capable of transforming a green vegetable structure—a leaf of a well-known type—into the mineral substance that you know as coal. I turn from this specimen and take up a large block of ordinary coal. On handling a cubical fragment I find that I can touch it on its shining lateral surfaces without producing any material defilement of my fingers. But I discover on the upper surface a layer of matter every touch of which does defile and blacken the hands; on examining this grimy surface with a microscope, I see that it is entirely made up of small fragments of wood. This is a layer of what is technically called mineral charcoal. You may sometimes have seen an old post or dead stump of a tree far advanced in decay. You may have noticed how it broke up into little square fragments, each, generally speaking, about half an inch in diameter. A similar disposition to break up into cubical atoms will be seen in the half-burnt log blazing on the Christmas hearth. Some similar process of decay has taken place here, and this blackened surface of the coal which always defiles our fingers, consists of a layer of such fragments. In the thickness of a small block there will be scores of these layers of wood. The microscope shews that each fragment is not a portion of a leaf, but of true wood; and yet, as in the case of the leaves, we see that these fragments are turned into genuine coal. We thus learn that Nature, by powers with which she has been endowed, is equally capable of turning leaves and wood into coal. I could shew you other specimens on the table, demonstrating that Nature is also capable of turning bark into coal; and what she can do in the case of one leaf, of one fragment of bark, and of one piece of wood, she is equally capable of doing with a cart-load, or with any conceivable quantity

of such materials. If she can do it with one atom she can do it with all, and she has done so. I think I have now succeeded in demonstrating to you the exceeding probability that coal was originally a vegetable substance. The next question that meets us is, In what shape and under what circumstances have these vegetable materials been brought into their present condition? Most of you have probably heard enough of geology to know that a very considerable portion of the surface of the earth's crust is made up of varying layers of rock superimposed one upon another; layers of rock which water has been the great agent in bringing into the positions where we now find them. You know that when muddy water is allowed to stand still in a vessel, the mud suspended in it sinks slowly to the bottom. That which will take place in a vessel upon your table also takes place in the sea, in the lake, and in the still reaches of the river. Such processes have gone on throughout all time, and often on the most gigantic scale. Seas have thrown down their sediments, which have accumulated, layer upon layer, at the bottom of the water, over geographical areas thousands of square miles in extent, and to incredible thicknesses. Lakes have, in like manner, become filled up with similar sediments. Rivers, which in some parts rush rapidly over their beds, in others, where the deepening of the bed has made the current less powerful, move slowly and sullenly; and inasmuch as the carrying power of the water is proportionate to the velocity of its motion, the moment that velocity is checked, in any way, it loses a corresponding amount of that carrying power. This check may be occasioned by the widening or deepening of the bed of the stream, or by some accidental impediment interrupting its course; as the "bars" of rivers are accumulated where the mouth of the river suddenly opens out into the wide, still ocean. However produced, all checks to the onward flow of the current cause the sediments to sink. Thus are produced layers of mud, of sand, of gravel, of stones, of whatever the river or ocean current may be transporting, the coarseness of the materials conveyed depending on the force with which the currents moved. This tendency of water to become

"silted up," if the processes described were allowod to go (sufficiently long, would ultimately bring the floor of a ?al or ocean up to its surface. Sooner or later your lak would become stagnant marshes, your oceans vast shallo lagoons, and a large portion of the earth would thus l covered by a thin layer of water in which there were ı deeps. Counteracting agencies have prevented the results from taking place. Disturbances of the crust (the earth, occurring in all ages, have produced inequaliti in the depth of the sea and in the heights of mountain Some portions of the earth's crust, even whilst I speak t you, are being lifted up, others are being depressed. N(only so, but these changes take place over very wide area. Hundreds, and even thousands of square miles in on part of the globe are being gradually and slowly raise(whilst other equally extensive areas in other regions ar being depressed. In accordance with this idea, I may sa broadly that those parts of the earth's surface on whic coral islands and coral growths are actively increasing are in a sinking condition, as Darwin has demonstrated t us. On the other hand, wherever you have volcanic peak in a state of activity, you have the opposite action goin; on. When the geologist appeals to risings and fallings o the earth's crust on a vast scale, some people are apt t say he is appealing to his imagination. But this is not so He is speaking of changes which he knows to be goin; on on the surface of the globe at the present day, an(the remark applies equally to the formation of deposit from water—"aqueous" deposits, as they are technicall called. Let me here utter one word of apology for being obliged to use a few hard and technical words. Wheneve I can convey scientific ideas to your minds in good, plai Saxon English, good, plain Saxon English you shall have But occasionally terms are needed for which our Saxor forefathers provided us with no substitutes, and therefore we are obliged to fall back upon such as are furnished by the classic tongues of Greece and Rome; and I regret tc say that the cases of such necessity are very frequent in science. But the further justification of their use is a very simple one. You must recollect that the language

of science is not for Englishmen alone. It is for the cultivated intellects of every nation upon the face of the earth; and, consequently, it must be so framed that the learned German, the Hindoo, the Chinese, and at some future time the native of Timbuctoo, must be able to understand the terms employed as well as ourselves. But since to-night I am chiefly speaking to a British audience, I shall not introduce more of these terms than I can help. The depressions and elevations of which I was speaking have continued to be produced through an indefinite number of years; I won't attempt to say how many they may have been, because we have no data enabling us to determine that point. But I think I may now venture to affirm, without fear of contradiction, what it would have seemed a great heresy to affirm fifty years ago, that they may more probably be numbered by millions rather than by thousands.

After these changes had gone on through vast periods of time—periods during which every part of the known surface of the earth had been more or less modified and covered by stratified rocks—the age arrived in which our British coal-beds were formed. For a long time we supposed that no coal-beds had existed in rocks older than those in which you find them in Scotland; than, for example, your coal-beds of Burdiehouse, of Fifeshire, and of your own more immediate neighbourhood of Glasgow. But we now know that there was a yet earlier coal-field. You have in Scotland a vast deposit, which one of your most illustrious countrymen has made so classic. You can boast of many great names, but there is none enrolled in the annals of Scotland that for honest enthusiasm, for broad intelligence, and especially for the most eloquent use of good, plain Saxon English, will ever stand higher than that of Hugh Miller. Hugh Miller has shewn us that Scotland is now composed of groups of rocks set in a framework of what geologists call the Old Red Sandstone. This Old Red Sandstone once covered a great portion of your country to a vast thickness; but most of it has again been removed by "denudation"—that is, by the mechanical action of ice and water. This Old Red Sandstone belongs

to the period immediately preceding the "Carboniferou age, or age of coal. But we now know that even then the existed, in the western hemisphere, coal forests just magnificent as those which existed at the time when t coal-beds of Burdiehouse were being accumulated; so th thousands and tens of thousands of years before the peri when our geological forefathers supposed coal plants an coal deposits to have first existed, the labours of my frie Dr. Dawson, of the M'Gill College, Montreal, in Canad have shewn that there were forests in his part of tl world equally magnificent, and that they have left behi coal-bearing deposits equally important, with those of o own island. Hence we must no longer limit such deposi to the true "Carboniferous" rocks.

The next question which I have asked now seeks i answer; a question equally appropriate whether the coal-beds belong to the Old Red Sandstone deposits Scotland and Canada, or to the newer deposits of your ow Coal formation; in both cases we equally find beds of co varying from a few inches to several feet in thicknes and we have seen that these beds represent vast accu mulations of vegetable matter. How have such quan tities of vegetation been brought together, so as to b capable of producing deposits often ranging over hill an dale for many miles of continuous extent, and with com parative uniformity of thickness? The old idea was tha vast vegetable rafts had been accumulated in the bed of th sea, the materials of which they consisted having been firs carried thither in the shape of drift-wood. It was known t the earlier geologists that large rivers, like the Mississippi brought down in times of flood immense quantities of vege table matter—trees that had fallen into the water by th wasting of the river banks, or conveyed by the floods whicl overspread the forest-clad plains through which the rive flowed. It was found further that these masses of vegeta tion floated about on the sea in the shape of huge rafts until they became saturated with water, when they sank to the bottom. It was supposed that such vegetable masses became overlaid with deposits of mud and sand, and ulti mately converted into coal. But when men came to conside

that a thin bed of coal, not more than a few inches in thickness, often retained the same uniform thickness over many square miles of area, it became clear that no drifted timber could ever be expected to accumulate in a layer sufficiently uniform in quantity, or be distributed with sufficient equality of thickness, to result in a bed of coal of this kind. Men then turned their attention to peat-beds in search of an explanation; and some of the Continental geologists, with a very near approach to truth, began to think that peat-bogs, such as we see in our own country, were the true representatives of the beds of coal. We know how peat-beds have been formed; and without entering into a detailed comparison which our time will not admit of, I may say that difficulties presented themselves which made it impossible to accept the peat-bog theory. Our ideas on this

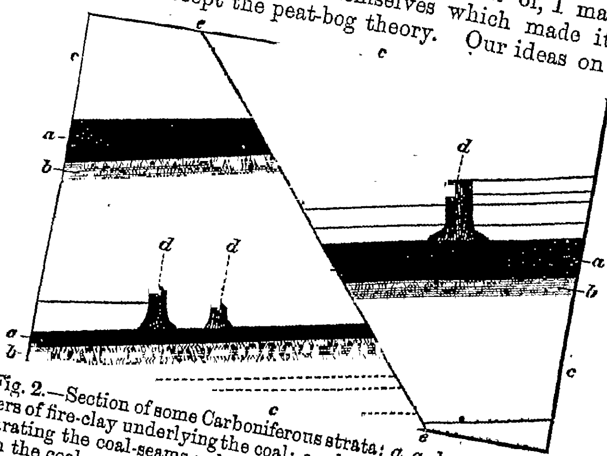


Fig. 2.—Section of some Carboniferous strata: *a*, *a*, beds of coal; *b*, *b*, layers of fire-clay underlying the coal; *c*, *c*, beds of shale, sandstone, &c. separating the coal-seams; *d*, *d*, fossil trees standing perpendicularly upon the coal-seams; *e*, *e*, a fault by which the upper coal, on the left, has been thrown down to a lower level on the right of the fault.

subject were in a foggy and unsatisfactory state when two very important discoveries were made—one in Cornwall and one in Lancashire. The late Sir William Logan, so long the distinguished head of the Geological Survey of Canada—a man of whom probably many of you may never

have heard, but whose name will always stand high in geological history—was then an earnest, hard-working geologist in Cornwall. He there noticed the important fact, that wherever there was a bed of coal, that coal rested upon a very peculiar bed of clay, which in England we know by the name of fire-clay, because it is the clay of which fire-bricks are made. On comparing Logan's observations in Cornwall with what we saw in our collieries in Lancashire, there was everywhere found to exist the same relationship. The bed of fire-clay invariably underlies the bed of coal. This relationship is shewn in Fig. 2, in which *a*, *a*, represent beds of coal, and *b*, *b*, the subjacent fire-clay. A constant relationship of this kind was clearly not the result of accident, but involved some important meaning. The next discovery is represented in the same diagram by the letters *d*, *d*. When the railway between Manchester and Bolton was made, it was found that some fossil trees of the kind represented in Fig. 3 were standing upon the coal in a perpendicular position. But their roots had evidently struck through the coal, and extended themselves into the clay beneath the coal. We have magnificent models of these trees, of the natural size, in our Owen's College Museum at Manchester. At the time in question, the present distinguished president of the British Association, Sir John

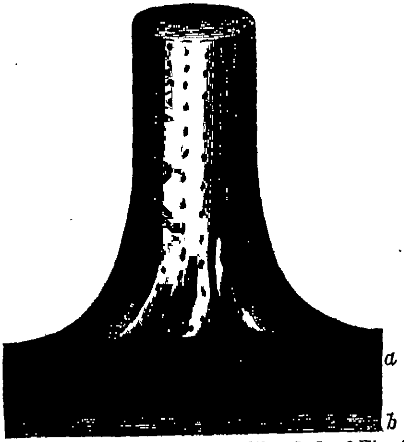


Fig. 3.—Stem of a Sigillaria, like *d*, *d*, of Fig. 2, standing upon the coal, *a*, with the subjacent fire-clay, *b*.

Hawkshaw, was the engineer of the Bolton line. Having the eye and heart of an intelligent geologist, he saw the significance of these trees, and took proper measures for their preservation. Not only were the casts taken to which I have already referred; but a shed was erected over the trees to protect them from decay, though I fear that the latter precaution was unavailing, and that the trees are now mouldering away under the destroying influence of the atmosphere. This discovery naturally connected itself not only with that already made by Sir William Logan, but with a second one, to which attention was called by that distinguished geologist. He found that in the clay under the coal there invariably existed immense quantities of fragments of a plant, which in those days was known by the name *Stigmaria*. Fig. 4 represents a fragment of this *Stigmaria*. This plant, in the condition in which we usually find it, looks like a bit of a tree that has suffered from a fit of the small-pox. On studying such specimens in detail, we find that these curious little pustular spots, *a*, are merely points from which certain slender rootlets, *b*, were originally given off—such rootlets being arranged in an extremely regular order. At that time we did not know that these were rootlets—indeed, we were very much

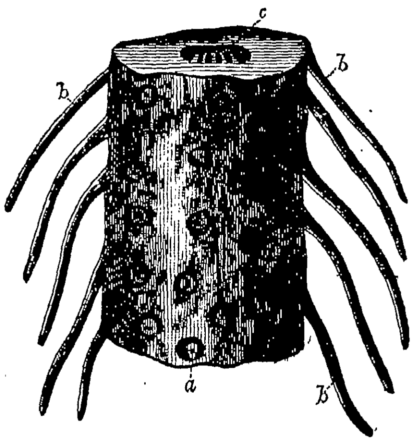


Fig. 4.—Fragment of a Stigmarian root: *a*, scars from which rootlets, like *b*, *b*, have been accidentally detached; *c*, the inner vascular zone surrounded by a thick bark.

perplexed to know what these *Stigmaria* were. We found their branches running away in radiating lines, extending fifteen or twenty feet from a dome-shaped centre. Everybody was perplexed with this plant. It was supposed by many to have been a floating water-plant; that these little rootlets, *b*, were its leaves, and that the small impressions, *a*, the points from which similar leaves had fallen. But finding that the trees standing on the coal had obviously plunged their thick roots through the coal into the fire-clay below, and the further important discovery being made by my friend Mr. Binney, that these Stigmarian structures were neither more nor less than the prolonged roots of the Sigillarian stems, it became evident to everybody that the trees in question must have grown where they stood; that the fire-clay, filled with their Stigmarian roots, was the soil on which the trees had flourished, and that a coal-seam represented the site of a more or less ancient primæval forest. Thus the drift theory received its death-blow. Other facts of the same kind were soon met with in various parts of the world—especially in Nova Scotia—shewing that the new theory explaining the sources of our British coal-beds was of world-wide application. But the further question now arose, What was the exact relation of the coal to the trees which must have over-shadowed it? No one that has taken a walk through a pine forest would have any difficulty in giving a probable answer to this question. Go into a pine wood of forty or fifty years' duration, and kick your foot into the ground. You do not find there ordinary earth. You have the leaves of pines, the cones of pines, fragments of the branches and twigs of pines, decayed and broken off, the former being what is called "deciduous," and the latter having been thrown down by the storms of winter. You find this vegetable soil to be of considerable depth surrounding the roots of the living pine trees. It is the accumulated product of the fallen portions of the trees that constitute the forest. When once pointed out, the resemblance between the vegetable soil that accumulates in such a forest, and the coal-beds of which I have been

speaking, became so obvious that there was no longer any room for doubt, either that the plants had grown in the spots where the coal now is, or that this coal was merely a thick bed of vegetable soil, which had accumulated through successive ages by the fall of portions of plants that grew there. But while the larger trees thus furnished a part of the growing mass of earth, you will readily understand that smaller plants would also live and flourish on the surface of the soil. Go into such a wood now, and you find growing under the trees various smaller plants, especially ferns. Here we have all the materials essential to the formation of a bed of coal.

But we have still to account for the existence of forty or fifty such beds of coal, one above another, sometimes separated by narrow intervals, and at others with thick intervening strata of shale or sandstone separating them widely apart. Such sandstones are merely layers of hardened sand, as the beds of shale are, in like manner, merely hardened layers of fine mud. How did these aqueous deposits come there? We know that when they were accumulated water was at work. We only find an explanation by falling back upon the doctrine of areas of elevation and of depression, to which I have already referred. There is no doubt that these coal-beds were accumulated on areas of depression. After a time the forest-clad land sank under the water; whether it was salt or fresh water is now a matter of discussion; but I have little doubt that both seas, rivers, lakes, and estuaries shared in the work of burying up the layer of vegetable soil. Now trees, as a rule, do not like living in water. Many of those which constituted the Carboniferous forests were probably adapted for living in a swampy soil, but they would not be likely to live in a deep lake or in an ocean. At the same time their dead, half-submerged stems would continue to stand long after they had sunk below the water. The consequence was that the stems of the trees whose bases were below the water-line became surrounded by layers of sand and other materials, which accumulated year after year, until at last this earthy material rose to the surface of the water. The central portions of the stems rotted

ving mere cylinders of hollow bark encased in he stems, thus weakened, would yield to storms, upper portions would be blown down. The de- od of the interior of the submerged stem would out, whilst the water which removed the vege- gments would wash into the vacant cavity sand along with shells, bones of animals, and such ed fragments of other trees as would be heavy sink rather than float. In time, the action of sterious agency, of which we are altogether produced once more at the surface of the shoal of blue clay in which these ancient plants evi- lighted more than in any other soil. You know ar as soil is concerned, cabbages have their likes ces, and so it was with these Carboniferous trees. liar kind of mud which ultimately became con- o fire-clay, was the only one in which they appear lourished. Other kinds of mud produced shale stone; but if seeds found their way to such, they o take root in the uncongenial soils, but on the fire-clay such seeds germinated, and rapidly his mud over with a thick layer of vegetation. successive processes were repeated again and til a mass of rocks, varying from 4,000 to 10,000 ickness, was produced, and it is this collective rocks which we call the Coal measures.

ng that I have succeeded in shewing you how has been accumulated, there next arises the How is it that we have such important dif- in the qualities of coal? I will venture to say few ladies in the room who do not take a interest in this part of our inquiry, whatever ands may do. But I will go a step further there is one part of the community that ought to take such an interest, and that is the ves. I remember some years ago reading a book tic economy which pointed out a very significant course, every young wife who knows her duty make home comfortable for the young husband, gst the most important elements in the comfort

of the early home is the power of making that home attractive when the husband returns from his business toils at the close of the day. The shrewd writer to whom I have referred pointed out that, on entering the house on a cold day, to take up the poker and stir up the fire is an instinct inherent in human nature. Dealing with the practical aspect of the question, the writer virtually said to the young wife,—"Take care to have ready for your husband a fire with a good crust on the surface, that will burst into a cheery blaze as soon as he thrusts the poker into it." It is simply impossible that this pleasant little result could take place unless you have a good coal to deal with. Hence my affirmation that the qualities of coal have a practical interest for the younger wives. I need scarcely say that there are many bad coals and some very good ones. Some of the bad ones won't blaze at all. Some of the good ones blaze extremely well. What is the reason for the difference between the two? Before explaining this point, let me call your attention to the following table, which you will find published in the admirable little *Manual on Chemistry*, written by my distinguished colleague in Manchester, Professor Roscoe. It gives the chemical composition of the several objects named in it, exclusive of the ash left by combustion:—

	Hydrogen.	Carbon.	Oxygen and Nitrogen.
Wood Fibre, . .	5·25	52·65	42·10
Irish Turf, . .	5·88	60·02	34·10
Cologne Lignite, .	5·25	66·96	27·76
Wigan Cannel, . .	5·85	85·81	8·34
Newcastle Hartley, .	5·61	88·42	5·97
Welsh Anthracite, .	3·38	94·05	2·57

You know that if you burn vegetable structures you drive off certain elements, but there is left behind a visible ash. This table shews you the relative amounts of the gases thus driven off. There is a column for hydrogen, the gas which we burn in a modified shape; one for carbon, familiarly known to us in one of its forms as charcoal; and one for oxygen and nitrogen, the two gases which form our atmosphere. If you take wood fibre as found in an ordinary piece of wood, you see

from the table that the carbon constitutes about one-half of its substance. As you descend the scale through turf and lignite or half-formed coal, the carbon increases in proportion to the other elements, until you come to the bottom of the list, where you have the non-bituminous anthracite or culm coal of South Wales, in which you find that there is 94 per cent. of pure carbon. I need scarcely tell those familiar with chemistry that a coal like the Welsh anthracite would not do for the young wives of whom I have spoken, because no amount either of heat or of pokering would make it blaze freely. The flame is produced by the hydrogen gas of the coal, and if the hydrogen is not there the blaze will not be forthcoming. This hydrogen was originally in the vegetation which has been slowly and gradually changed into coal by a succession of chemical and mechanical processes. Whither has it gone in the case of the bad coals? It has disappeared in a variety of ways. Coal has probably been millions of years in forming. You see that the Irish turf contains more hydrogen in proportion to its carbon than coal, but less than new wood. Subjected to a slow decomposition affecting atom after atom, through vast durations of time, the vegetable mass has inevitably allowed some of its more volatile gases to escape into the atmosphere. You know how difficult it is to bottle up these gases from the readiness with which an escape takes place if you have the smallest imperfection in your gas-pipes. If the vegetable matter which was being converted into coal were not firmly enclosed beneath a layer of gas-proof clay before the chemical changes to which I have referred to took place, some of these gases would inevitably be lost. The carbon not being volatile, had not such a tendency to fly off; the quality of the coal would thus depend very much on the nature of the rocks which overlay the bed of coal. If these consisted of a very solid bed of compact clay, then the gases liberated during the slow chemical alterations which the coal underwent, would be retained as in a closed bottle; consequently, as the elements of which these vegetable masses consisted, after being partially severed, were re-combined in their new and final condition of coal, all these volatile

gases would again be locked up in the coal, giving to it excellent qualities for ordinary burning purposes. But if, on the other hand, you had a porous roof overhead, your coal would suffer just as the qualities of a bottle of pure Glenlivet would suffer, if you merely closed its neck with porous cotton wool instead of with a good firm cork; the strength of the whisky would very soon fly off, and so it is with Nature's coal-bottling. A coal with a covering of loose porous sand would be liable to lose all its best gases, and with them most of its best qualities, so far as household purposes are concerned.

I have, I think, shewn you how one coal is liable to be made better than another; but I am now going to shew you practically the difference between a good coal and a bad one, by exhibiting to you a charming example of the former class. I daresay you all know something of the occasional difficulty of getting a fire to light. If you had such a coal as this you would have no such difficulty. [The professor here lighted at a candle a piece of coal, which blazed up quickly with a steady flame.] The material which I am now burning has some degree of historic interest. It is the well-known Boghead coal, from which the first manufacture of Paraffin oil was made. Why does it burn in this way? Because it contains such an enormous amount of hydrogen gas. You probably remember that some years ago a very celebrated trial took place in Edinburgh. The great question in dispute was the true nature of this Paraffin Boghead mineral. The owner of the estate had sold the "coals" upon it; but he contended that the Boghead mineral was not coal, therefore he had not sold it. Scientific witnesses were brought together almost from the ends of the earth, and the question put to them was—"Is this coal, or is it not?" A certain number of these learned gentlemen said "Aye," and a certain number said "No"—a not uncommon phenomenon in law-courts. Some of the witnesses said it was not coal, because it was dull and not shining. But oddly enough, at the same time, a similar trial was going on in America about another coal, and there the opposite view was taken. It was urged that the American sample could not be coal,

because it *was* shining. The judge at Edinburgh, taking a practical commonsense view of the matter, decided that the mineral was coal, and Messrs. Young & Binney reaped the advantages of this judicial decision. I have shewn you a good coal; I need not shew you a bad one—you can get that anywhere.

We will now proceed to consider what the plants were out of which coal was formed. Here we have no difficulty, because of late years our knowledge of these plants has made such extremely rapid advances, that we are almost as familiar with the forests of the coal ages as my botanical friend behind me (Professor Dickson) is with the forests of Scotland. What were these Carboniferous forests? Were they composed of trees like those that grow around us now? Certainly not! Were there any Oaks, or Ashes, or Elms? Nothing of the kind. The forests were composed of plants that now grow only as dwarf herbaceous objects, chiefly creeping on the ground. Botanists travelling over Scotch mountains are sufficiently familiar with the group of plants known as club-mosses, or *Lycopods*. These Lycopods now rarely rise more than two or three feet from the ground, even in their most developed tropical forms. They have not strength to support themselves at any greater elevation, therefore they creep like other weak things that cannot sustain themselves. The relatives of these Lycopods constituted the principal and most conspicuous objects in the forests of the Carboniferous period; but instead of being the lowly objects which we see living around us, they then reared their heads as lofty forest trees. Here is a small stem (shewing specimen), a part of a tree such as is represented in Fig. 5. But how is it that these trees were able to grow to such large dimensions? How is it that trees now only represented by creeping herbs could grow to the height of 100 feet, as some undoubtedly did? Some of these trees have been found with a circumference of twelve feet at the base of the stem. I have not time to go into the minute details of the internal anatomy of these trees, but I will shew you briefly that there existed in their stems a mode of growth which does not exist in the stems of their living representatives.

Trees of that lofty growth required buttresses to support them, and these buttresses were provided by a beneficent Nature. Fig. 6 represents a transverse section of the stem or branch of one of these trees in a very young state. At *a* you have a cellular pith surrounded by a cylinder of vessels, *b*, derived from the leaves. It will be seen that,

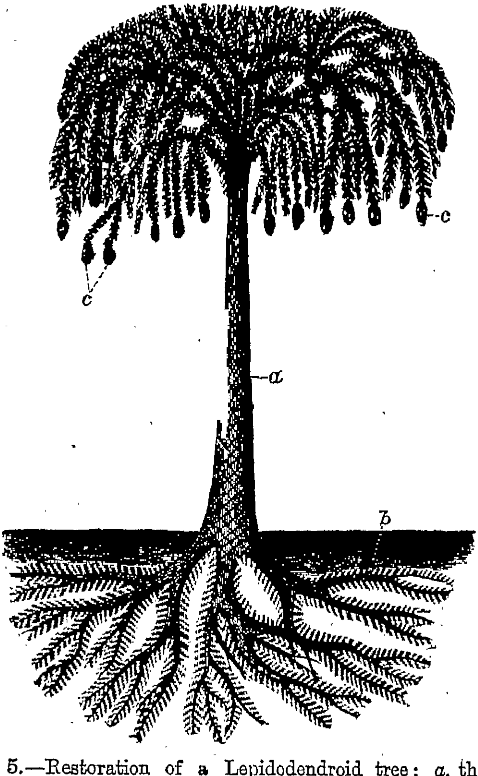


Fig. 5.—Restoration of a Lepidodendroid tree: *a*, the stem, slender above but thickened below; *b*, Stigmarian underground roots, like Fig. 4; *c*, Lepidostrobi or spore-bearing cones hanging from the ends of the ultimate twigs.

in this transverse section, these vessels are not arranged in radiating lines. This vascular ring is encased by an inner bark, *c*, and an outer one, *d*, the latter sustaining the bases of the leaves, *e*. The radiating lines crossing the bark represent the bundles of vessels going from the vascular cylinder, *b*, to the leaves. In all that is essential this stem-structure corresponds with what we find in the living Lycopods; but when we come to the larger and more matured fossil stems, we discover that age has developed an altogether new series of vessels, which have a very different mode of growth and arrangement to those constituting the inner cylinder, *b*. Fig. 7 represents the plan of a transverse section of the central part of one of those more matured stems. Here again *a* represents the pith, and *b* the inner vascular cylinder; *c* is a portion of the innermost bark with the vascular bundles, *d*, going to the leaves. But we now find the inner cylinder, *b*, surrounded by an outer one, *e*, in which all the vessels are arranged in lines which radiate from within outwards. We further see that this ring has grown steadily by successive additions to its outer border, in consequence of a vital activity of the innermost bark at this point. We also discover that these radiating lines of vessels are separated

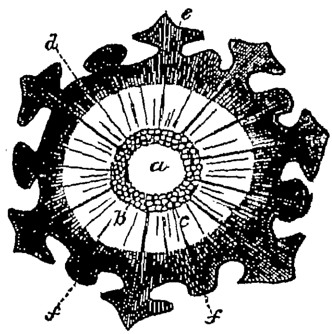


Fig. 6.—Transverse section of a young branch of the Lepidodendron, Fig. 5: *a*, the central cellular pith; *b*, a ring of barred vessels derived from the leaves, and not arranged in radiating lines; *c*, an inner bark of delicate cellular tissues; *d*, an outer bark containing a cylinder of strong resisting fibrous tissue; *e*, transverse sections of the bases of leaves; *f*, sections of similar leaves at a lower point of their attachment to the bark. The radiating lines passing through the bark are vascular bundles, extending from the vascular cylinder, *b*, to the leaves.

by thin vertical layers of cellular tissue, constituting wl botanists are familiar with under the name of medullε rays. In fa we have here regular syst of exogeno growth, whi only differs points of det from what is n seen in the oal elms, and simi trees composi our mode forests. Nothi of this ki occurs in t plants of tl class which ε living now. V therefore coı to the conclusi that when th existed as tre during the Cε boniferous ag they were pı vided with mode of grow which they lc when they became degraded and ceased to require it, aı when their place in the forest was taken by other aı very different trees.

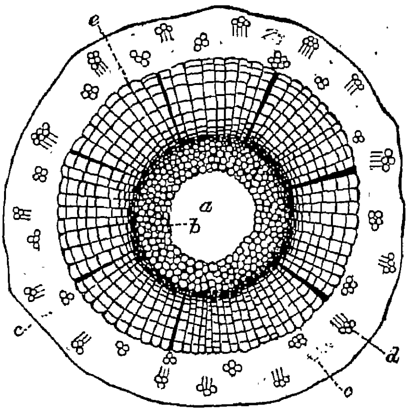


Fig. 7.—Central portion of a similar section to Fig. 6, only taken from the lower part of a more matured branch or stem: *a*, the pith; *b*, the inner vascular cylinder, identical with *b* of Fig. 6; *c*, the innermost layer of the bark; *d*, vascular bundles coming from the vascular cylinder *b*, to the leaves; *e*, an outer cylinder of vascular tissue, the vessels of which are arranged in exogenous order—*i.e.*, in radiating wedges separated by medullary rays, and which grow exogenously or by additions made to their exterior margins.

Associated with these Lycopods we also find anoth group of plants, which were the Carboniferous repr sentatives of the modern horse-tails. You are ε familiar with the horse-tails, or Equisetums that gro in almost every pond and marshy spot. In the presei day the largest of these plants are only five or six fe high, and even that is a very unusual height. The foss

forms from the coal are called *Calamites.* For a long time we were only familiar with these objects in the form of casts, composed either of shale or sandstone, like Fig. 8, where we have eight joints, each of which is grooved longitudinally. Sometimes these specimens are invested by a bark-like film of coal. It turns out that these specimens are not the real plants; but before saying what they are, let me direct your attention to Fig. 9, which represents a vertical section, or rather one-half of the lower end of what I believe the plant to have been when living. At *a* we have an externally smooth bark, varying in thickness with the age of the plant. At *b* there is a woody zone, which is seen in the transverse section at the upper end of the drawing to consist of a ring of wedges of vascular tissue, separated by medullary rays. Within this vascular zone is an exceedingly thin film of cellular pith, but which stretches entirely across this part of the plant at each joint, *c*, a line of hollow cavities, *d*, thus forming a series of transverse divisions separating from each other, which occupy the centre of the stem. In fossilisation these cavities became filled with sand and mud, forming casts like Fig. 8. The transverse constrictions, *e*, of these casts were caused by the transverse layers of pith, Fig. 9, *e*, which separated one fistular pith-cavity from another. The longitudinal grooves of each joint were produced by the projection into the pith cavity of the thin inner margins of each of the vertical woody wedges, Fig. 9, *c*. When these specimens are invested by a layer of coal, that layer represents what was once either the vascular zone or that tissue and the bark conjoined, but the greater part of the elements of which have disappeared in the process of fossilisation. Fig. 10 is a restored sketch of the probable aspect of these Calamites when growing. We know much about their

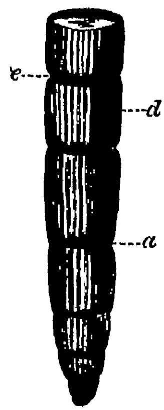
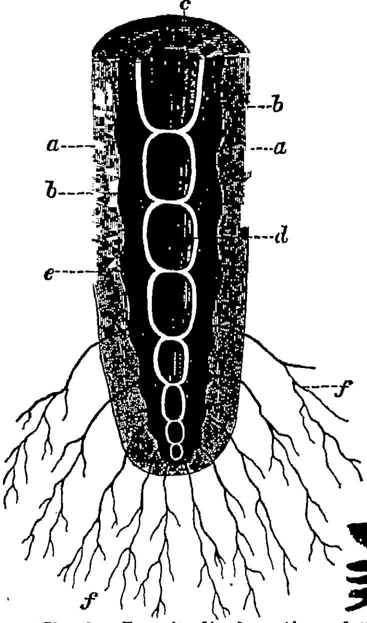


Fig. 8.—Ordinary aspect of a sandstone cast of part of a Calamite.

roots, stems, and leaves, but have yet but very scanty information respecting their reproductive organs. But



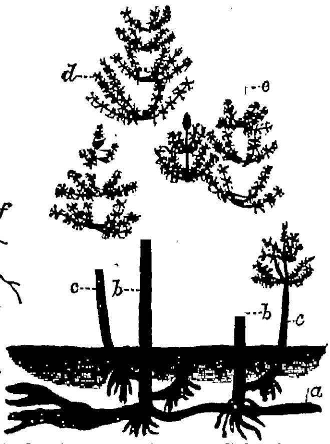


Fig. 9.—Longitudinal section of the lower end of a Calamite, exhibiting the structure of its interior: *a*, a thick bark, smooth on its external surface; *b*, a vascular zone, the vessels of which have a peculiar arrangement, but which have an exogenous arrangement and mode of growth; *c*, the separate vascular wedges of the vascular zone *b*, as seen in transverse sections; *d*, a vertical series of fistular cavities in the interior of a very thin pith, the latter being represented by the white line. This pith forms transverse partitions which extend entirely across this part of the stem at each node, *e*, separating contiguous fistular cavities from each other; *f*, *f*, branching rootlets of Calamites.

Fig. 10.—Restored group of Calamites: *a*, subterranean creeping stem, sending up ærial stems, *b*, which again give off lateral shoots at *c*, *c*; whorls of leaf-bearing branches are given off at the upper part of these stems, *d*; whilst spore-bearing organs (strobili) probably existed at the extremities of the stems, as at *e*.

whilst Fig. 8 represents the usual form in which Calamites occur, I have specimens in my own cabinet in which the stem displays a thick wood surrounding the pith, the former being also enclosed in a thick bark; the stem in these specimens has been at least some thirteen inches in diameter. There are other specimens in existence in which the original stem must have been from three to four feet in diameter, judging from the magnitude of the sandstone cast of the pith cavity, and the relation which that cast bears in these large examples to what I find in other specimens in which both the wood and the bark are preserved. I have no hesitation in saying that many of these Equisetaceous plants, which are now of comparatively dwarf size, once rose up to a height of thirty or forty feet, and constituted some of the forest trees of the period in which they lived.

Let me now call your attention to one peculiarity which we find in the coal formed from some of the trees which I have been describing. If we make a section of a piece of coal perpendicularly to its horizontal surface, we often find it full of sections of little flattened discs, like very minute coins, as is represented in Fig. 11. There is no

Fig. 11.—Microscopic vertical section of a fragment of coal containing numerous depressed macrospores, or female spores, of a Lepidodendroid plant. Fig. 11*a* represents the upper surface of a macrospore flattened vertically. The triangular ridge in its centre is the flattened remnant of the angle resulting from four spherical spores mutually compressing each other so as to form a common spherical mass, in the centre of which all the four spores are in mutual contact.

doubt whatever as to what these discs are; but to understand them we must look at the fructification of the club-mosses already referred to. I have told you that these plants have no flowers; but they have little reproductive bodies, which we call spores. Each plant forms little spikes, which consists of a number of leaves (Fig. 12, *b*), gathered round a shortened stem, *a*. At the upper surface of the base of each of these leaves there are little capsules, *c*, *d*, inside which there are what botanists call spores. But there are two kinds of these spores—large ones, called macrospores, and small ones, called microspores. Nearly all our British Lycopods only possess the latter form; but we have one British species, not uncommon in your Scottish bogs—the Selaginella Selaginoides, which resembles a large number of the tropical forms of these plants, in possessing both kinds of spores. Fig. 12 represents a vertical section of the central portion of one of these latter spikes of fructification. It will be seen that in it the uppermost of the little capsules, "sporangia" or spore-cases, as they are called, are crowded with very minute spores, whilst the lower ones each contains about four such spores; but these latter are of large size—"macrospores," as they are called, which simply means *big* spores, as "microspores"

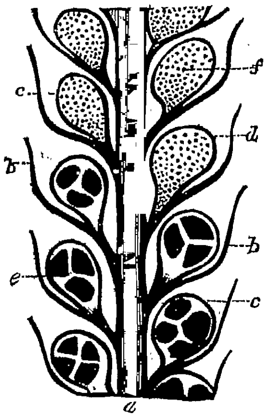


Fig. 12.—Vertical section through the central part of a strobilus or spore-bearing spike of a recent Lycopodiaceous plant (Selaginella): *a*, the vascular axis; *b*, *b*, leaves; *c*, *c*, sporangia, or *spore-cases*, springing out of the bases of the leaves, *b*; *d*, the uppermost sporangia filled with male microspores, *f*; *e*, the lower sporangia, each usually containing four macrospores, or female spores, identical with those seen in Figs. 11 and 11*a*.

means little spores. So far as function is concerned, the microspores represent the male fertilising elements of the pollen of a flower, whilst the large spores did the work of the ovule and seed. The flattened objects so numerous in the coal, and represented in Fig. 11, are neither more nor less than these macrospores. Their existence in coal was pointed out by Henry Witham half a century ago, though he was ignorant of their nature. The enormous number of them in many coals shews how abundant the plants must have been from which they were derived; but we have already seen that the Lepidodendra were the most common of the trees of the Carboniferous forests, and there is no doubt that it was from them that these spores were derived. In Fig. 5 many of the dependent branches are represented as having "cones" or "strobili" (*c*, *c*) hanging from their extremities. These "cones" are commonly known as *Lepidostrobi*, and are the fruiting or reproductive organs of the plant to which they belong. How many of them were provided with both large and small spores, as represented in Fig. 12, we do not yet know, since in most specimens of fossil Lepidostrobi, the "sporangia" or spore-cases are quite empty, the spores themselves having been shed and fallen to the ground, adding to the inflammable materials of the coal. But we do know that many Lepidostrobi possessed both macrospores and microspores; and the great numbers of these spores existing in the coal renders it probable that most of them were so furnished.

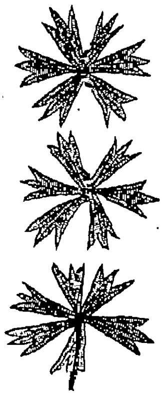
It is an interesting circumstance connected with this part of our subject that the spores of recent Lycopods have long been used in theatres for the purpose of producing artificial lightning, their value for this purpose arising from their high degree of inflammability. Though I am not prepared to recognise in the fossil spores the chief, still less the only, source of the inflammability of good coals, there is no doubt that they contribute their share towards giving the mineral its valuable qualities. I am told by my friend, Mr. Charles Calvert, that Lycopod spores are now thrown overboard by stage-managers, electricity being used for the same purpose. The botanist has

been displaced by Sir William Thomson and the electricians.

I need hardly say that ferns were very abundant in this Carboniferous age; but we have the further interesting fact, that not only at Autun, in France, have what are known as tree-ferns long been found in a fossil state, but I have recently obtained in Lancashire unmistakable evidence that tree-ferns also existed in England.

We now have to consider another group of plants that contributed very materially to the formation of the coal-beds. I mean plants allied to the pine tribe. You know that pines bear their fruits in the form of cones. In our part of the world a popular appellation for these cones is "fir-bobs." Why they got this odd abbreviation of a common Christian name, I never could find out; but they have it. Though we find an immense number of stems in the Coal measures that evidently have borne a very close resemblance to the stems of your Scottish pines and other fir-trees, we have not yet found in the Coal measures a solitary example of a true Pine cone. It therefore becomes clear that cones, if they existed at all, were very rare. But, on the other hand, we have recently brought to light such a multitude of seeds which evidently belonged to this or some allied class of plants, that it becomes a matter of extreme perplexity to know on what stems they grew. My friend, M. Grand Eury, labouring in the coal district of St. Etienne, in France, has found some forty or fifty kinds of these seeds in one bed alone. These St. Etienne seeds are now undergoing investigation at the hands of M. Adolphe Brongniart, who is about to produce a work on the subject. At the same time, I am now working on similar objects discovered in the coal-beds in Lancashire. I have also found some interesting seeds of the same type in the Burntisland beds discovered by Mr. Grieve, and which have already played so important a part in contributing materials for elucidating the organisation of the Coal-measure plants. The most remarkable feature in many of these seeds is this: You know that in the living flower the pollen, liberated by the anther, falls upon the pistil, and through its aid reaches the seed,

upon which it exercises a fertilising influence that makes that seed productive. But in these fossil seeds we discover the pollen in a very different position. We find in the majority of them a flask-shaped cavity in the interior of the seed, situated at its upper end, between the kernel and the "testa," or outer covering of the seed. Thus the pollen has not only fallen directly upon the seed, but has been carried into its interior, where a large open cavity was prepared for its reception. Brongniart has come to the conclusion that these seeds belong to a



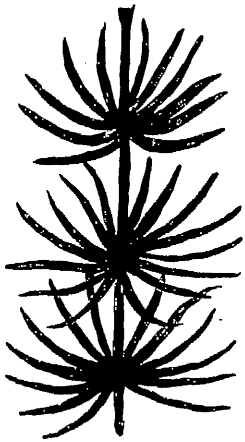
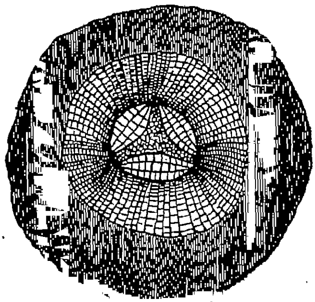


Fig. 13.—Twig with three verticils of leaves of Sphenophyllum.
Fig. 14.—Twig of Asterophyllites with three verticils of linear leaves, each having *one* central nervure.

Coniferous class of semi-tropical plants allied to the pines, and known as Cycadeæ. On this point, however, I do not yet see my way to a decided opinion. From the number and size of these seeds obtained conjointly by M. Grand Eury and myself, it is evident that we have had, at the Carboniferous age, some fifty or sixty species of seed-bearing plants, of which we have as yet been unable to identify the stems and leaves. But it is perfectly clear that, at the

time in question, there lived forests of seed-bearing plants more or less allied to your firs; or more probably they have had a closer relationship to the yew and to the Salisburia, a plant found in China, and commonly known as the Gingko tree. These seeds make an important addition to our knowledge of the flora of the Carboniferous age, but one which requires much further investigation. They will, however, abundantly repay the time and toil bestowed upon them.

I would call your attention further to the transverse sections of the stems of some other coal-plants, illustrative of their peculiar structure and of the way in which that structure has been preserved in a fossil state. In Fig. 13 we have a twig of a Sphenophyllum with three of its pretty verticils of wedge-shaped leaves; and in Fig. 14 we have a similar twig of the closely allied genus Asterophyllites. Fig. 15 exhibits a transverse section of the stem of these two plants, shewing the central vascular ax s



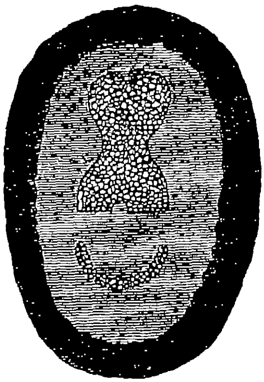


Fig. 15.—Transverse section of the stem of an Asterophyllites, shewing the remarkable arrangement of the central vascular area, enclosed in a thick bark. The corresponding section of Sphenophyllum is identical with this Fig. 15.

Fig. 16.—Transverse section of the petiole of a fern, *Rachiopteris duplex*, with its two vascular bundles enclosed in a double cellular bark.

surrounded by its two layers of cellular bark. Fig. 16 represents a transverse section of the foot-stalk or petiole of the leaf of a fern, with its curious vascular bundles encased in a bark. Then in fig. 17 is the transverse section of the stem of another plant, from our Lancashire district, which rejoices in the name of *Lyginodendron Oldhamium.* It is specially characterised by having numerous undulating

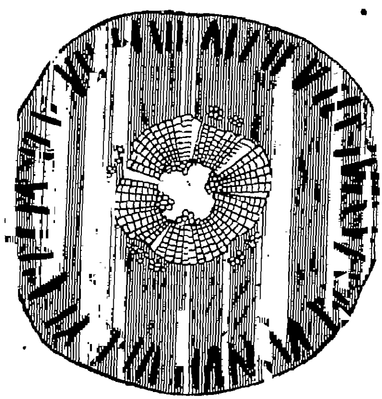


Fig. 17.—Transverse section of a stem of *Lyginodendron Oldhamium,* a plant of unknown affinities.

vertical plates of hard fibrous tissue, which, in the transverse section of the stem represented in the drawing, look like the Roman figures on the face of a church clock. Fig. 18 represents a transverse section of another petiole of a fern—the Myelopteris. This plant has long been regarded as the stem of a Palm. However, we have now very clearly ascertained that it is merely the foot-stalk of a fern-frond; consequently, Palms have to pass,

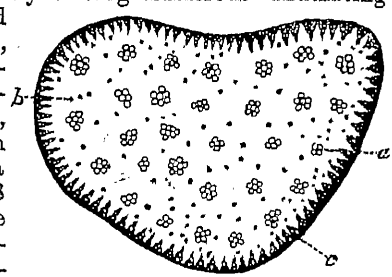
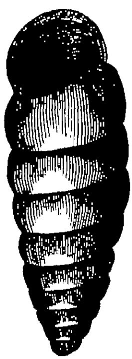


Fig. 18.—Transverse section of the petiole of Myelopteris, a Marattiaceous fern; *a*, vascular bundles passing through a mass of cellular parenchyma; *b*, numerous small gum-canals; *c*, fibrous peripheral bands, forming a sub-epidermal layer.

for the present, out of the history of the Carboniferous age. If they existed, we have yet to obtain the proof of their existence.

I shall not detain you with many remarks about the animal life of the Coal measures. Animal remains are not uncommon in the coal deposits, but they do not come within the scope of this lecture. My friend, Dr. Dawson, of Montreal, in Canada, long ago found in the interior of certain fossil trees the remains of a reptile. You will say this is a queer place in which to find such objects, but I will tell you directly how they most probably came there. Then we find the remains of fish somewhat abundantly even *in* the coal. A large number of these fishes were sharks of a high organisation. Probably half of these fishes were of this class. To find anything resembling the other half you must go to the River Nile, or to the American Mississippi. In the latter river we find a fish called a Garpike or Lepidosteus, with nearly square bony scales overlapping each other, like the tiles on a house-top. It is a representative of the largest class of fishes existing in the ages terminating with the chalk rocks, and which are known as Ganoid fishes. The only fishes which we find in the coal-beds associated with the sharks, are of this order. The vast number of these Ganoids and sharks in the coal-beds strongly inclines me to the belief, that many of the coal deposits must have been accumulated in the sea, or at least in brackish, estuarian waters. But a curious difficulty has been brought to light within the last few months, and which stands in the way of this idea. An Italian observer, who has obtained the ash of coal by a very careful process of burning, has found in that ash an immense number of microscopic siliceous organisms that belong to a group of objects known by the name of Diatoms. We find them, at the present time, both in the sea and in fresh water. They are really minute cellular plants, each cell of which consists of Silica or flint. The curious feature about these Diatoms that this Italian Count has found in the coal is, that every one of those which he has discovered is a fresh-water species. This looks uncommonly as if coal had either been formed

in bogs or marshes saturated with fresh water, or that fresh water had, from time to time, flooded the forest areas, and thus left these Diatoms behind in the wet, spongy soil. The discovery is too recent for us to estimate its full value; but I am quite prepared to allow its importance up to a certain point. Probably the conclusion that will be arrived at is, that both fresh and salt water played their part in producing the Coal measures—that the soil of the marshy forests was frequently saturated with fresh water, but that submergence brought the sea over these areas when the ordinary deposits of sandstone and shale were accumulated; but that it was when the sea-level had once more been reached, and fresh water again came into action, that the fire-clay accumulated and prepared the soil for the reception of the spores and seeds of the land plants. I may further mention that there were numerous reptiles in these Carboniferous waters



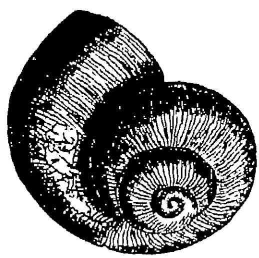


Fig. 19.—*Pupa vetusta*, Fig. 20.—*Conulus priscus*, } Two air-breathing land shells—copied from Dr. Dawson's *Story of the Earth and Man*, p. 139.

which were chiefly of the "newt" type—some of them very large crocodilian-looking fellows, but still not rising above the Batrachian tribe of frogs and newts. When we come to the shells of the upper coals, we again have to

deal with perplexing facts. In the lower Coal measures the shells are all undoubtedly marine, but in the upper ones we have aquatic shells so different from all living ones, that we cannot say with certainty whether they were marine or fresh-water; but, besides these, Dr. Dawson has found in Nova Scotia air-breathing land shells, very similar to what we see creeping up the trees of our woods at the present time. Figs. 19 and 20 represent two of the shells found by Dr. Dawson in the very centre of the stem of a hollow tree. One of these is a true snail, and the other belongs to a group very closely allied to the snails. But now comes the question,—How did these shells and these bones of reptiles, to which I have previously referred, get into these trees? Your townsman, Mr. Wünsch, a few days ago sent for my inspection a very beautiful transverse section of a large tree stem. In it, as is so commonly the case, I find preserved the tough fibrous tissue of the outer bark, but all the more internal structures are gone, and their place is occupied partly with clay and partly with portions of smaller stems of various kinds, that had no necessary relation to the tree itself. Some of these are branches that may possibly have belonged to the tree; some are leaf-stalks of ferns, and some are bits of Stigmarian roots. How did these things get inside this cylinder of bark? Most probably as the dozens of shells and the bones of reptiles got inside. We have already seen that in many cases these trees probably grew in marshy places, and in places where successive floods of muddy water were depositing thick layers of silt, raising the surface of the ground higher and higher, so that each tree growing upon that ground had the base of its stem gradually buried more and more deeply in sand and mud. Possibly, in some cases, they may have been so immersed to the extent of many feet, and yet continued to flourish. Sooner or later these stems perished; but owing to their peculiar lax structure in these Cryptogamic stems, the inner tissues decayed sooner than the outer bark, because these outer barks were composed of the strongest materials—those that would be the last to give way to

the forces producing vegetable decay. There is no doubt whatever that these central portions did decay, leaving an outer cylinder of bark standing in close contact with the surrounding earth. When one of these recurring floods of which I have spoken overflowed the land, whether from a slow, steady sinking of the land, or from a sudden water-flood, we know not, this water at once filled the hollow bark-cylinder of the stem. The interior being all rotten—broken up into those little rectangular bits I have already referred to, and which, as we have seen, constitute the great mass of the mineral charcoal—the water would at once cause these light fragments to float out, whilst heavier pieces of wood, impelled by the stream, would tumble in. In like manner insects, bones of reptiles, and the shells that lived upon the outside of the bark of the trees would be liable to be washed in; and thus it is that we find so many objects altogether foreign to the internal life of these trees imbedded in their very centre.

But I see that my time is now gone—I am perfectly well aware how hurried and sketchy has been the outline that I have been enabled to give you. But I trust that I have not altogether failed to redeem my pledge of shewing you that even so common an object as coal has connected with it a history which is capable of profitably arresting the attention, and is worthy of occupying your thoughts. If I have succeeded in my great object I shall be glad. What I want to impress upon the men and women of this large audience is the dignity of knowledge when possessed for its own sake, and the supreme interest that attaches to everything which the Almighty has made. I want them to look upon all things that have proceeded from the Divine hand with reverence. To see that they are *not* cursed, but beautiful and holy; and then as they learn to reverence God's works, and the forces which He employs as His instruments, their reverence will inevitably rise to Him who is The Lord of all power and might.

KENT'S CAVERN:

ITS TESTIMONY TO THE ANTIQUITY OF MAN.

Though I have had the pleasure of visiting Scotland several times, though I have stood on the summit of Ben Nevis, and have thus been for a while the most elevated man in your country, though I have the pleasure of the acquaintance of several gentlemen resident in Glasgow, yet, ladies and gentlemen, I am a stranger amongst you. Though I have visited Glasgow once before, it was fully thirty years ago, and I believe the Glasgow of this day is rather an improvement upon that. Being thus a stranger, I propose to throw myself on your mercy at once, and to tell you two of my defects as a lecturer. You will discover some of the rest before the evening is over. First, I am a very rapid speaker. I regret it; but believe that when a man has unfortunately hair of the colour of mine he is not likely to get into a new groove. I will, however, do my best. Secondly, though I have read a good many papers in your country, I have never delivered a lecture in Scotland until this evening, and I am afraid I shall appear before you rather as an English lecturer than, perhaps, as a Scotch one. We are in the habit in England of supposing that there is some young fellow in one corner of the room to whom it is necessary to explain all technicalities. Though there can be no doubt that you are fully acquainted with the technicalities to which I allude, it is not unlikely that, from force of long habit, I may nevertheless explain them, and perhaps you will be so good as to put it down to my acknowledged failing.

I need not inform any one present that there is on the south coast of Devonshire a beautiful inlet of the English Channel, known as Torbay, having on its northern shore the town and harbour of Torquay. About a mile eastward

from that harbour, and half a mile north from Torbay, there is a charming valley, running north and south, and having on its west side a small hill of *Devonian* limestone, belonging, that is, to the formation next older than your *Carboniferous* limestone in this country. It reaches a height of 230 feet above mean tide level at Liverpool, and terminates on the eastern side in a vertical escarpment some 30 feet high; and in the face of this cliff there are two apertures 54 feet apart. They are the entrances to one and the same large, dark, dirty hole known as Kent's Cavern or Kent's Hole; and to hear me talk about Kent's Cavern for an hour or so, you have been so good as to leave your pleasant firesides this evening.

Surrounding the hill of which I have spoken there are other eminences, but they are not of limestone, though they belong to the same *Devonian* formation, the equivalent, or nearly so, of your Old Red Sandstone; and they reach a greater elevation. One of them, about half a mile to the north-west, attains the height of 450 feet, and is the loftiest eminence in our neighbourhood. These loftier hills are composed of grey schist and dark red grit. It may be of importance to us to remember this fact when we get a little farther on.

It would seem that the cavern was never discovered. In other words, it has always been known. Judging from the objects which were found mixed up with earthy matter in the floor, the cavern was visited in Mediæval times, in Saxon times, in Romano-British times, and in Pre-Roman times. Objects connected with all these periods have been met with, and therefore it is probable that the cavern has always been known by the dwellers in the neighbourhood.

The earliest date connecting itself with the cavern is 1604, and the connection is in this way:—There are in various parts of the cavern masses of Stalagmite, having inscriptions on them, consisting of names, initials, and dates, and the earliest of them is dated 1604. Besides this there are 1615, and several others in the early part of the seventeenth century. We have reason for believing —and I may as well dispose of that question now— that these inscriptions are perfectly genuine. In such a matter it is of considerable importance that we should be

quite sure that they are trustworthy; and I shall tell you, a little later on, about a man who made Kent's Cavern a study some fifty years ago and wrote a description of it (which description is in my keeping as Honorary Secretary of the Torquay Natural History Society), and these inscriptions were described by him in such a way as to convince me that they are really as old as they profess to be. I repeat that the earliest date connecting itself with the cavern is 1604. One of my legal friends at Torquay has a map, or rather plan, of the property in which the cavern is situate, and which is known as the Manor of Torwood; and in that map, executed in 1769, one portion of the property is named "Kent's Hole Field." We may take it for granted, then, that Kent's Cavern was very well known at that time, and had been known long enough to give a name to that part of the property. The earliest description, if description it may be called, is one to which Sir Walter Trevelyan has recently called my attention. It occurs in the eighth edition (1778) of a work entitled *A Tour through the Island of Great Britain*, written by Defoe, and edited by Richardson the novelist. The description is this,—"In the parish of Tor is a very remarkable place, called Kent's Hole, not mentioned, as I can find, by the writers on this county (Devon), though perhaps the greatest natural curiosity therein" (vol. i., p. 347). That, then, is the earliest account of the cavern we have, so far as is known. You will observe there is a little disadvantage in this—at least, there is a possible disadvantage; for some persons may be inclined to say that since the cavern has been so long known, it is possible that visitors have ransacked it and turned it topsy-turvy, and that we cannot expect to find it in its original condition. Those, however, who are acquainted with the cavern will attach no importance to any such remark.

I trust no one will ask the origin of the name; for, first, I don't know anything about it; and, secondly, I might be tempted to tell you the following absurd story:—It is said that many years ago an animal—some say a dog, and some say a hawk—was taken into the cavern and lost, and was subsequently found in the county of Kent;

from which it was concluded that there was a subterranea passage from the cavern in Devonshire to the South-Easter County of England. I don't ask you to believe that, for don't believe it myself; but if you are not satisfied, yo can, no doubt, invent an origin for the name as good a that yourselves; but that is all I can tell you. Severa descriptions besides that just quoted, about 1797 and 1803 are very well known, and the cavern appears to have bee a place visited by almost all who came into that part o the country.

The earliest researches in it were undertaken by Mr Northmore of Cleve, near Exeter, in 1824. He believed that the worship of Mithras was generally carried on in caverns, and thought it not unlikely that it had been conducted in Kent's Hole; and thither he came to discove evidences of it. I have generally found that when an enthusiast is resolved to find evidence he succeeds; and as it is possible that I may be an enthusiast, you had bette be upon your guard lest I should impose upon you. Before Northmore went to the cavern, however, he had met with Dr. Buckland's *Reliquiæ Diluvianæ*, and thought he might, to use his own words, "kill two birds with one stone," and seek bones in the cavern, just as Buckland had sought bones and found them in the cavern of Kirkdale, in Yorkshire. He thought he succeeded in finding evidence of the Mithratic worship; but, be that as it may, he certainly succeeded in finding bones, and was thus the first to shew that the cavern was worth the attention of palæontologists. In 1825 he made another visit to the cavern, and was accompanied, as a matter of idle curiosity only, by a Roman Catholic priest resident at Torquay, whose name was John MacEnery. His fame is for ever connected with that cavern, but he died before he became famous. I regard it as one of my sacred duties to speak in the highest terms of the labours of the Rev. John MacEnery in that cavern. He commenced, in 1825, a series of researches, about which I shall tell you directly. He put himself into communication with Dr. Buckland, who was, to use the language of Lloyd's, "A1" at that time on cavern questions; and he and Buckland, it is probable, were to produce jointly a work on Kent's Hole.. In 1841 MacEnery died, and I

attended the sale of his effects at Torquay. In that sale there was, I remember, an old tea-chest, filled with papers and odds and ends, including splinters of bones, which was bought by a Torquay tradesman, who gave a small sum for it. A few years afterwards, Professor Owen and others having in the interval deplored the loss of the manuscripts, he turned out the contents of the tea-chest, and there were the missing papers. They were ultimately presented to the Torquay Natural History Society, of which I happen to be honorary secretary. A few years ago I transcribed the whole, and gave it to the world as MacEnery left it. There were some peculiarities of spelling, &c., and some *lacunæ*, but I regarded the whole as sacred, and printed it in its entirety and as I found it. (See *Trans. Devon. Assoc.*, vol. iii., 1869.) The quotations which I shall give you as I go along are derived from that source. He went first, as I have said, out of mere idle curiosity, had never been in a cavern before, and was not a geologist; but, being capable of receiving the true *virus*, he became a geologist at once and in earnest. Speaking of his first visit, he says:—"The passage being too narrow to admit more than one person at a time . . . the company entered in files, each having a light in one hand and a pickaxe in the other, headed by a guide carrying a lantern before the chief of the band. I made the last of the train, for I could not divest myself of certain undefinable sensations, it being my first visit to a scene of this nature." (*Op. cit.*, p. 208.) There had been no preparation for cavern researches on his part. Not being satisfied with the way Mr. Northmore went to work, he betook himself to a small recess alone, where he began to dig, and succeeded in discovering bones and teeth; and you will see from the passage which I am about to read that he knew nothing in the world about palæontology, or, indeed, science generally. He says:—"They were the first fossil teeth I had ever seen, and as I laid my hand on them—relics of extinct races and witnesses of an order of things which passed away with them—I shrank back involuntarily. Though not insensible to the excitement attending new discoveries, I am not ashamed to own that in the presence of these remains I felt more of awe than joy." (*Op. cit.*, p. 210.) Most of us have been students.

We have all more or less burned the midnight oil, and v have seen rise up before us in our studies unwished, u bidden, and almost, as we have thought, unholy, a new id that has awed us, for we have felt that it was destined strangle some of our former and most cherished opinion He was not only thus successful in discovering bones ar teeth, but you will see from the next quotation that he soc made another discovery of perhaps still greater importanc "In the summer of 1825 Dr. Buckland, accompanied l Mr. Northmore of Cleve, visited the cave of Kent's Hole : search of bones. I attended them. Nothing remarkab was discovered that day, excepting the tooth of a rhinocer and a flint blade. This was the first instance of the occu rence of British relics being noticed in this or, I believe, ar other cave. Both these relics it was my good fortune find." (*Op. cit.*, p. 441.) Towards the close of 1825 l commenced a series of systematic observations—systemat for that day. Science insists on still more systemat observations now-a-days. As I have seen his exploratio characterised as superficial, I take every opportunity of spea ing of the exceedingly conscientious work that that mi performed there. MacEnery recognised four deposits the cavern. First, huge blocks of limestone, that had fro time to time fallen from the roof, and which were in mo cases cemented together with stalagmite. Second, benea and between these blocks, a black mould, consisting main of vegetable matter, probably blown in through the entrance Third, below that, a sheet or floor of stalagmite, from a me film to five feet in thickness. Fourth, still lower, a thic deposit of loam or cave earth. These were the deposi with which he became acquainted, and he knew of nothin more; and Sir Charles Lyell knew of nothing more, so fa as Kent's Hole is concerned, when he published his gre work on the *Antiquity of Man.*

I have several times used the word "Stalagmite," an it is time to explain it to my young friend in the corne The roof of the cavern is of limestone, of course, an through it, in rainy weather, water percolates, slowly i most cases, but sometimes more rapidly. That water coi tains carbonic acid—the gas which is given off in an effe vescing draught, and which is given off by a burning lim

kiln, which the unfortunate fellows who go down to the bottom of a newly emptied porter vat inhale and sometimes die from. It is by that carbonic acid that the water dissolves the limestone which constitutes the roof. It reaches the inner surface of the roof, and hangs there as a drop. You come into the cavern and hear a drop here and a drop there, and you know what process is going on. The limestone has been dissolved overhead, and as the water falls it brings a particle of the limestone to the floor, where it is precipitated or left by the water. It sooner or later forms a little boss—more or less conical generally. Thence it flows away, and meeting that flowing from other such bosses, a sheet is ultimately formed which covers the entire floor. This is Stalagmite. A part of the carbonate of lime or limestone dissolved by the water remains on the roof, and by increase hangs down as a pendant. That pendant is Stalactite. The difference is not one of composition, but one of place only; the Stalagmite forming the floor, and the Stalactite depending from the roof. The stalagmitic sheet or floor cannot be formed more rapidly than the limestone is dissolved, which again is, to use a mathematical term, a function of the amount of carbonic acid in the water. Indeed, it cannot be formed *so* rapidly as the limestone is dissolved overhead; for, as I have said, a portion is retained on the ceiling. It follows, as a matter of course, that the formation of stalagmites is usually a very slow process indeed.

I have told you what deposits MacEnery recognised. I will now tell you what animals he found in the lower deposit—the cave earth. He found the mammoth, rhinoceros, horse, ox (two species), gigantic Irish deer, red deer, reindeer, hyæna, tiger, wolf, fox, bat, weasel, pika, mole, rat, vole (three species), bear (three species), and *Ursus cultridens*, which we now call *Machairodus latidens*. He includes a "tiger" you observe; but there is no evidence that there ever was a British tiger. I need not tell this audience that there has been and still is a British lion. No one can tell the difference between the skeleton of a lion and that of a tiger, unless he happens to have this part of the head on which my finger is situated. Through the kindness of Mr. Young, I had the pleasure

of seeing to-day, in the Museum at the University, ti; skulls and lions' skulls; and you have all seen in a yc child's head that the parts of the skull in very early are not united here. There is a line of union run across here that we call a *suture*. As you look at lion's head, you will see that the suture takes a some\ three-pointed form—a straight line uniting these two o or extreme points passing just over the middle poin the lion's head; but in the tiger's head the line passe front of the middle point. Unless he has that part of skull, no naturalist can distinguish between the skel of a lion and a tiger. Every portion of an identifi feline skull of this large size that we have met wit unquestionably that of a lion. We, therefore, think are safe in ascribing the teeth to the same species—tha to the lion also. MacEnery also mentions the "Pika." is a small animal, not larger than a good-sized rat—a without a tail—and its analogue at the present da found in Siberia. The cavern species is also known as *Lagomys spelæus*.

I have told you that MacEnery discovered a flint in ment. He subsequently discovered many. I will read to you a short extract describing the care he toc determine the exact position of these implements:—"] ing cleared away on all sides the loose mould anc suspicious appearances, I dug under the regular crus stalagmite], and flints presented themselves to my h This electrified me." Ladies and gentlemen, this was years ago; and I can remember when, even nearer tc present day, it would have been rather more agreeable man to have encountered an electric shock, than to] gone into the world and stated openly that he had fc evidence of man in such a position as that. "I called attention of my fellow-labourer, Mr. Aliffe, and in presence extracted from the red marl [or cave earth] ar and lance heads." Don't attach much importance to t definite expressions. For these words "arrow" and "la heads," read "implements." "I instantly proceeded to excavation inside, which was only a few feet distant in same continuous line, and formed part of the same plate layer]. The crust [of stalagmite] was about two feet th

ıady; the clay [or cave earth] rather a light red. About ree inches below the crust the tooth of an ox met my e [an extinct ox]. (I called the people to witness the ;t), and not knowing the chance of finding flints, I then oceeded to dig under it, and at about a foot I dug out a ıt arrow-head. This confirmation, I confess it, startled ;. I dug again, and, behold, a second! I struck my mmer into the earth a third time, and a third arrow-ad answered to the blow. This was evidence beyond all estion." So I should have thought, but even the scientific ırld did not believe him. "Dr. Buckland is inclined to ,ribute these flints to a more modern date, by supposing ıt the ancient Britons had scooped out ovens in the .lagmite, and that through them the knives got admission the diluvium [or cave earth], and that in this confused .te the several materials were agglutinated together. ithout stopping to dwell on the difficulty of ripping up a id floor, which, notwithstanding the advantage of under-ning and the exposure of its edges, still defies all our orts, though commanding the apparatus of the quarry, am bold to say that in no instance have I discovered idence of breaches or ovens in the floor, but one con-uous plate of stalagmite diffused uniformly over the .m. It is painful to dissent from so high an authority, d more particularly so from concurrence generally in his ıws of the phenomena of these caves, which three years' rsonal observation has in almost every instance enabled) to verify." Dr. Buckland's opinions—and I speak it :her unwillingly, for he did splendid work for geological ence—his opinions on MacEnery's discoveries retarded the ogress of anthropology for fifty years. Nevertheless, the ection, by scientific men, of the Kent's Cavern and ailar facts was of great service, for it rendered it absolutely cessary to undertake cavern researches in the way they ; now undertaken, and has thus given a value to them ıich they otherwise would not have.

Mr. MacEnery made two assumptions :—First, that there ıre no remains of extinct mammals in the stalagmite. cond, that flint implements were not to be found in the wer depths of cave earth.

In 1840, Mr. Godwin Austen, then resident in Devon-

shire—and perhaps no other man has ever done such good work in the geology of that county—published the following words as expressive of his opinion, or rather his discoveries:—"Arrow-heads and knives of flint"—read "flint implements"—"occur in all parts of the cave and throughout the entire thickness of the clay; and no distinction, founded on distribution or relative position can be observed whereby the human can be separated from the other *reliquiæ*." (See *Trans. Geol. Soc. London*, 2nd Series, vol. vi., part 2nd, page 444.) In 1846 the Torquay Natural History Society appointed a committee, consisting of Dr. Battersby, Mr. Vivian, and myself, to make some limited researches for the purpose of getting specimens for their museum. We found precisely the same things as were found by our predecessors—remains of extinct animals in the cave earth, and with them flint implements in considerable numbers. You want, of course, to know how the scientific world received these later discoveries. They simply scouted them. They told us that our statements were impossible, and we simply responded with the remark, that we had not said that they were possible, only that they were true.

Towards the close of 1857 a man named Philp, residing at Brixham on the southern shore of Torbay, purchased, from the Commissioners for the Enclosure of Waste Lands, a piece of ground, about a quarter of an acre, on the slope of a hill at Brixham, where, as you know, William III. landed. Philp's object was to excavate the limestone, and to build a series of cottages on the site. It happened that there was what geologists call a "joint"—a close fitting fissure—running through the rock, and the removal of a layer of limestone from this place revealed one day a hole in that joint large enough to admit a man's fist. The men, on returning from their dinner, missed one of the tools with which they bore holes for blasting, called in Devonshire a "jumper," and they supposed it had been stolen. A few days afterwards, on the removal of another layer of rock, it was found that the hole previously noticed was larger. It was a conical hole, expanding downwards, and large enough to admit a small man. The proprietor, going to inspect the hole, saw that it was not

of any considerable depth, and that at the bottom lay the missing tool. He went down to recover the jumper, and found himself in a gallery running into the hill. Calling for a light, he went into what proved to be a cavern, previously quite unknown, and having bones and a fine antler attached to the stalagmite, whilst others were sticking up through it. Here then was a cavern in which there had certainly been no ransacking. That was early in 1858. I went to Brixham almost immediately, and saw Mr. Philp. He said he intended to take out the bones and sell them. This I discouraged, stating that what scientific men desired was not bones merely, but to note the accompaniments, the associations, and the positions of the specimens; and I prevailed on him to give me the "refusal of a lease" in his cavern. To make a long story short, a countryman of yours, the late Dr. Falconer, interested the Royal and Geological Societies of London in the new cave, a committee was formed for its careful and thorough exploration, a lease was taken in it for three years, and we found there what MacEnery and others had stated they had found in Kent's Cavern. The whole work was placed under my superintendence, as being the only resident member of the committee, and we completed the work in one year. I had the pleasure of reading a paper on the subject to the British Association at Leeds, in 1858. I do not say, of course, that that paper revolutionised the opinions then held on the antiquity of man, but there can be no doubt that that cavern did. Falconer, Lyell, Prestwich, Ramsay, and others gave in their adhesion to the opinion that man was, indeed, an occupant of Devonshire in times far earlier than our fathers had supposed.

In 1864, when the British Association met at Bath, it happened there was a concurrence of circumstances that rendered it a favourable time for endeavouring to get a committee appointed by the Association and a grant of money to explore such parts of Kent's Cavern as still remained intact. We moved in the matter, got a committee appointed, with a grant of money, and set to work on 28th March, 1865. We have had annual grants from that time up to the present; and all being well, when we meet in

Glasgow in September next, we will have another g
carrying on the work.

Now, ladies and gentlemen, I want to convince y
you may trust to the narrative which I am to gi
The work was put under the general superintendenc
friend Mr. Vivian and myself, and I was appointe
tary. I have gone to Kent's Cavern every day of
from the 28th March, 1865, up to the present day, e
those rare instances when I am from home; and I h
the pleasure of taking into Kent's Cavern a great m
tinguished men, and amongst them my distinguishe
Sir William Thomson. I mention this by way of co
you that I am just about to speak of what my o
have seen and my own hands have handled. I thin
not be asking too much if you, at present, at any ra
you hear to the contrary, assume that in this ma
word may be taken. There is a malicious story
about Torquay, to the effect that one day I was u
go to the cavern, and my boots were met walkin
their own accord. You can do as you please, but
believe it myself.

Two questions were especially pressed on the atte
the committee. First, Is it true that flint implem
found in the cave earth in Kent's Cavern, commin
coeval with the remains of extinct animals? Seco
it true that the animal which Cuvier and MacEne
Ursus cultridens, and which we now call *Mac*
latidens, is really found in Kent's Cavern? for its
there would be a remarkable thing, seeing it is no
elsewhere in Great Britain, and, from its zoological a
it seems to belong to an earlier period.

This is a plan of Kent's Cavern on a scale of one
five feet linear. The blue is limestone, and the wh te
the blue is the cavern. You will perceive that the
consists mainly of two parallel divisions, which, s
roughly, have an east and west direction, and that t
considerable lateral ramifications. One of these
occupied us about thirteen months in exploring it.

I must tell you about the deposits we have m
First, then, and omitting the overlying blocks of li
already mentioned, there is the *Black Mould*, as w

at the top. That varies from three to twelve inches i thickness. It consists essentially of vegetable matter—leaves blown in through the entrances probably in th autumn. There is evidence of animals having made thei home there; evidence of bacchanalian parties having visite the cavern; evidence of human beings having for a whil made their home there; evidence of people having gon there and paid the guide, and lost a coin now and then Below that comes the stalagmite, which, from a peculiarit of its structure, we call the *Granular Stalagmite.* Tha varies from a mere film to five feet in thickness, as previ ously stated; and I may tell you in passing, that whereve the drip in wet weather is very copious, there the stalagmit is of great thickness; wherever there is very little drip th stalagmite is thin; and in those few places where there i never any drip at all, there is no stalagmite. It woul seem, therefore, that the lines of drainage have always bee the same as now. Below the granular stalagmite is th *Cave Earth,* the thickness of which at and near the entrance we know nothing about, but we have found its depth els where. It consists of a light-red clay, with about fifty pe cent. of small angular pieces of limestone. That is all w knew when Sir Charles Lyell published his *Antiquity o Man,* which almost seemed to frighten the world from it propriety. We have made discoveries since which, I hold have at least doubled that antiquity. Below the cav earth we find, as we get away south-westerly, anothe stalagmite, always thicker than the upper one. Wher the upper one was five feet the lower was twelve feet, an it had a totally different structure. It was eminentl crystalline, the crystals being generally in vertical prism We call that the *Crystalline Stalagmite.* Below that i another cave earth, but different altogether from th upper cave earth. Instead of the staple being light-re clay it is a dark-red sandy paste. Instead of the inco porated stones being small angular fragments of limeston they are subangular and rounded fragments of dark-re grit, such as the cavern hill could not have supplied. The must have come from the more distant heights, and, i present circumstances, could not have entered the caver by any means. We call that the *Breccia,* and know nothin about its depth.

Remember that we have to do our work in su
as not only to satisfy ourselves, but to convince al
of the truth of what we are asserting. We
vertical section down through the deposits, say
feet from the entrance, at right angles to a
line drawn horizontally from a point at the
to another at the back of the first chamber, in tl
tion, as it happens, of W. 5° N. magnetic. We dr
at right angles to the datum at eleven feet from the
so as to define or mark off a new "parallel" a f
Along this entire belt or parallel we take off tl
mould from side to side of the chamber, and
it carefully by candlelight *in situ*. Another m
it then to the door, and re-examines it carefully by
All the objects found in it are put into a box, which
bered, and a label is put in with them. We proc
the stalagmite in like fashion; we then come to
earth, where we are still more particular. We tak
simply a yard in length and a foot in depth—in
parallelopiped a yard long and a foot square in the
and termed a "yard." We examine that in like mar
what we get is put into a box, and so on yard af
and level after level to a depth of four feet b
granular stalagmite. All the boxes thus filled
the course of a day are sent to my house in the
I find in each a label telling me what yard, wh
what parallel, what series of workings, and what
the specimens belong to, so that I can, by ta
any specimen, say to half an inch whereabouts
plan it was found. Again, as it sometimes,
rarely, happens that a mass of earth falls dow
specimen slips out of the deposit unobserved, th
position of certain objects cannot be accurately det
All such specimens are placed in a box marked
tain." Further, we are careful not to give an
for any person to see the cavern. They can go
the guide, and be taken to see those parts of th
that have been explored, but not to where the
in progress, or where it is not yet begun, unless
panied by my friend Mr. Vivian or myself. Th
is we once did give an order to two young m

they foolishly put a Roman coin into the deposit, and our workman dug it out. I came by appointment to meet my young friends, when the foreman came aside to me and said—"This is very disagreeable to us. These gentlemen must have put this coin into the deposit. It is quite bright." I looked at it, and handing it to the gentlemen, said—"Will you be so good as take your coin. It has done all the work you intended." From that time we have passed a self-denying ordinance, never again to give any one an order to see the cavern. I hold that scientific investigation should not be undertaken with any theological bias, but that it must and should be undertaken with a religious regard for truth and accuracy; and hence the care we bestow and the restrictions we make.

To come to the objects we found in the deposits: First, in this black mould: Stones of various kinds; shells of hazel nuts, shells of snails, limpets, whelks, oysters, cockles, mussels, pectens, solens, and cuttle-fish; bones of fish, birds, seal, water-rat, rabbit, hare, goat, sheep, red-deer, short-fronted ox, brown bear, badger, fox, dog, pig, and man; whetstones; angular and curvilinear plates of slate, perhaps covers for earthenware vessels; pieces of smelted copper; bronze articles, including rings, a fibula, a spoon, a spearhead, a socketed celt, and a pin; flint "strike-lights;" numerous potsherds, including a piece of Samian ware; spindle whorls made of stones of various kinds, and some of them ornamented; a bone awl, a bone chisel, bone combs of the size and somewhat the shape of shoe-lifters, or shoe-horns, with the teeth at the broad end; amber beads; charred wood; a halfpenny of 1806, and a sixpence of 1846. Many of the artificial objects go back to Romano-British and Pre-Roman times. Hence the black band or uppermost deposit is worth two thousand years at least, and may be worth much more. Next, we come to the formation below the black mould—the granular stalagmite. In that we found comparatively few objects, but these are they:—Stones of various kinds; impressions of ferns; shells of cockle and cuttlefish; remains of bear, mammoth, hyæna, rhinoceros, horse, fox, and man; charred wood; and flint flakes and "cores," the cores being the remnants of flint nodules, from which flakes have been detached. I have found teeth of the mammoth,

teeth of the woolly rhinoceros, teeth of the cave hyæna, and teeth of the cave bear in the very uppermost part of that stalagmite; and a human jaw, with four teeth in it, at the base of the same deposit. I forgot when enumerating the successive deposits to mention a black patch which you see here in the section. It covers an area of about a hundred square feet, and is thirty-two feet from the nearest entrance. That black patch, as it is represented here, is just below the granular stalagmite—in some places in contact with it, and in others having a thin layer of cave earth between. That *Black Band*, as it is termed, consists almost exclusively of charcoal. It was, without doubt, the hearth of the old cave men. In that we found remains of ox, deer, horse, badger, bear, fox, hyæna, and rhinoceros; 366 flint implements—(some of you who are fond of coincidences may perhaps see a connection with the number of days in a leap year)—flakes and chips; a bone awl; a bone needle or bodkin, with a well-formed eye in it; a bone harpoon; burnt bones and burnt wood. The cave earth, the last drawn in that section, is the great mausoleum. In it we found—Teeth and bones of cave lion, lynx (?), wild-cat, cave hyæna, wolf, fox, isatis (?), glutton, badger, cave bear, grizzly bear, brown bear, mammoth, *Rhinoceros tichorhinus*, horse, wild-bull, bison, gigantic Irish deer, red deer, reindeer, hare, pika, water-vole, field-vole, bank-vole, beaver, and *Machairodus latidens*—(MacEnery was right then. It was suspected, as already stated, that some mistake had been made about the Machairodus in Kent's Hole. I had drawn up seven annual reports for the British Association, and had to state in each of them that we had met with no trace of this great carnivore; indeed, the eighth report was roughly drawn, when, lo and behold! on washing a tooth just sent me from the cavern, I saw with delight that peculiar serrature on it which unmistakably pronounced it to be an incisor of *Machairodus latidens*, and the negative evidence of nearly eight years shrivelled up at once in its presence, and was worth nothing)—whetstones, a hammer stone, lanceolate and ovate flint tools, flint flakes and "cores;" a bone pin, two bone harpoons, charred wood and charred bones, and coprolites of hyænas. Some of my friends have

a power of scepticism that I almost envy. They doubt these flint tools being of human workmanship. I make them, metaphorically, a present of the stone implements, however. But what do they say about these bone harpoons, this bone needle, this bone awl, and this bone pin? *They* were found in the same deposit, and as low as the flints were, and are certainly coeval with the extinct cave mammals. We come next to this lower or *Crystalline Stalagmite,* in which we found nothing but bones of bears. Even the hyæna did not appear. In the *Breccia* we found remains of bear, parts of two jaws containing teeth of lion, three teeth in part of a jaw of fox, and there in that deposit, older than the cave earth, vastly older—as it is decided to be by its infraposition, by its being made up of totally different material, by that thick bed of crystalline stalagmite which lies between them—there, too, we found evidence of man, in rude massive flint tools.

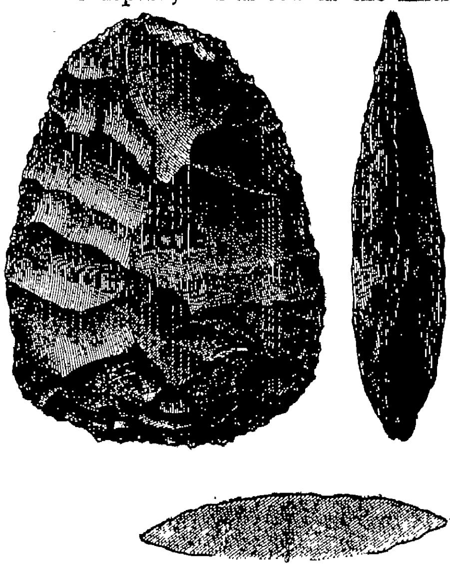


Fig. 1. ⅓

Mr. John Evans, a member of the Committee now engaged in the exploration of Kent's Hole, who, in his great work on Ancient Stone Implements, has figured many of the human industrial remains found in the cave earth,*

* See *The Ancient Stone Implements, Weapons, and Ornaments of Great Britain.* By John Evans, F.R.S., F.S.A., &c. 1872, pp. 444–466, figs. 386–408.

has kindly allowed his blocks to be used in illustrating this lecture. It has not been deemed necessary to make use of more than nine of these figures, which will suffice to give an idea of the typical specimens. The accompanying descriptions have been compiled from the *Cavern Journal*, the *British Association Reports*, and Mr. Evan's work, just mentioned.

Fig. 1 (No. 1,163 * in the *Cavern Journal*, and Fig. 386 in Mr. Evan's work) represents, on the scale of one-half, linear, an ovoid disc of grey cherty flint, carefully chipped on both faces, one of which is rather more convex than the other. It is wrought to a slightly undulating edge all round the perimeter, except at one spot on the side, where blows seem to have been given in vain in attempting to remove a flake, and its bilateral symmetry is sensibly perfect. The traces upon the edge of wear or use are but slight. It was found in the "Great Chamber," 53 feet from the southern entrance to the cavern, in the fourth foot-level of cave earth—the lowest to which it has been excavated—over which was the continuous floor of stalagmite about a foot thick; and was dug out in the presence of the Rev. W. Harpley, Mr. W. N. Row, and myself, Jan. 15, 1866.

Fig. 2 (No. 1515, *Cavern Journal*, and Fig. 389, Evans) is that of a specimen, on the scale of one-half, linear, which may be said to connect the ovoid and lanceolate implements; differing from the former in being pointed at one end and square at the other, but resembling them in being broadest not far from the middle. Its dimensions are considerably below those of the ordinary ovoid tools, whilst its breadth in proportion to its length is greater than in typical lanceolate specimens. It is of fine-grained cream-colour flint, and the patina covering its entire surface is more pronounced than in any other of the cavern

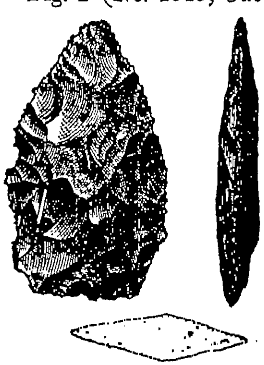


Fig. 2. ½

* The numbers quoted from the *Journal* are those, not of the specimens, but of the "finds" to which the specimens belong.

implements. Its faces are equally convex, and are chippe over their whole surface. Its bilateral symmetry is almos perfect, and from its shape it seems adapted to have forme the point of a lance; but the lateral and basal edges are i many parts worn away, as if it had been used as a sort o scraping tool, and it has lost its extreme point. It wa found in "The Gallery," 83 feet from the nearest externa

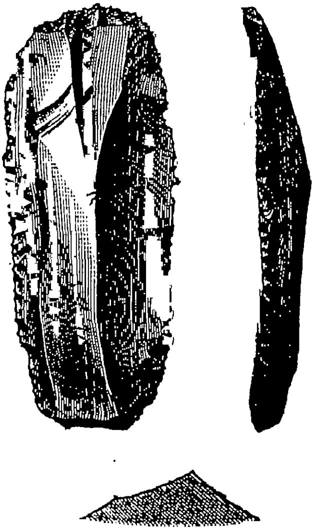


Fig. 3. 1/1

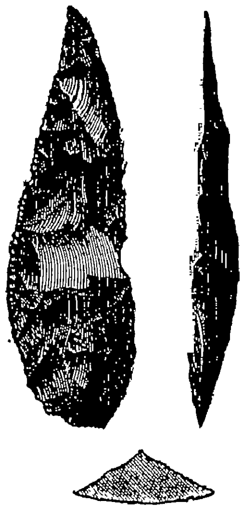


Fig. 4. ½

entrance, in the second foot-level of cave earth, beneath a thick and continuous floor of stalagmite, May 8, 1866.

Fig. 3 (No. 1832, *Cavern Journal*, and Fig. 397, Evans) is a full-size representation of a tool, formed from a ridged flake, and exhibiting marks of having been in use as a scraping tool, not only at one end, but also at the sides. It was found, with two other such specimens and a flint chip, in the first foot-level of cave earth, beneath the continuous stalgamite floor, 13 inches thick, in "The Vestibule," upwards of 30 feet from the northern entrance, November, 17, 1866.

Fig. 4 (No. 3869, *Cavern Journal*, and Fig. 391, Evans) gives views, on the scale of one-half, linear, of a remarkably

elegant instrument, made from a ridged or carinated fl
but having three facets at the butt-end and a l
secondary working on one side. At the butt-end the o
face of the flake, not of the nodule, has been left in
original condition; whilst the ir
face, as shewn in the figure, has
the original surface of the flake alr
entirely removed by secondary w
ing, and the edges have again l
retouched so as to make them e
and sharp. The butt-end is ch
like in form. It is of a sort of pie
flint, being partly of a white and e
where of a drab colour; and was fo
in the second foot-level of cave ea
beneath the continuous floor of sta
mite, which was 32 inches thick, in
"South-west Chamber," upwards
130 feet from the nearest exte
entrance, in my presence, July 4, 1
There were a few bones lying witl
and immediately below were thir
molar teeth of horse, a canine toot
hyæna, and a gnawed bone.

The specimens artificially wro
in bone are seven in number, of w
five only are figured in this lecture.
figures are all of full size.

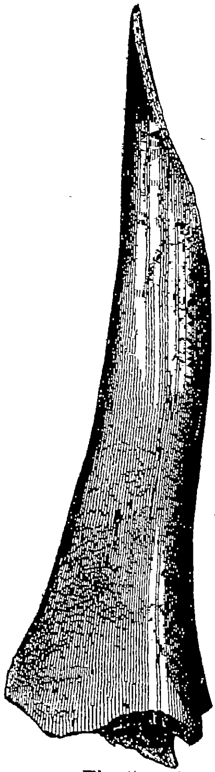


Fig. 5. ½

Fig. 5 (No. 1,835, *Cavern Jour*
and Fig. 407, Evans) is that of a l
awl, sharply pointed at one end. It
found, with several bones and five
chips, in the black band, about 40
from the northern entrance, below
continuous and unbroken floor of sta
mite, 16 inches thick, November
1866. The marks of the tool with which it was scraped
shape are distinctly visible on it.

Fig. 6 (No. 1,847, *Cavern Journal*, and Fig. 408, Ev
represents a bone "needle" having a well-drilled circ
eye, but unfortunately without its lower or pointed e
It is slightly taper in form and elliptical in transv

section. Its greatest diameter at the larger end is abou ·075 inch, and where broken about ·05 inch, so that it original length was probably about 2·55 inches. The ey is capable of receiving thread of about three-eightieths of a inch in diameter, or of the thickness of fine twine. Thi interesting specimen was found in the black band, beneatl the floor of granular stalagmite, December 4, 1866; bu at that time, being almost entirely enveloped in stalagmite, from which the broken end alone projected, it was supposed to be merely a small bone of no particular interest, and its true character was not discovered for nearly two years. During the interval, the "find," of which it was one specimen, had been placed by itself in a box, as in all other cases, and packed away in a room set apart for the cavern specimens. On 24th September, 1868, whilst I was preparing the osseous contents of a number o boxes for the inspection of Mr. Boyd Dawkins, one of the palæontologists on the Cavern Committee, the investing stalagmite fell off this specimen whilst it was in my hand and at once disclosed the true character of what had beer put aside as nothing more than a small ordinary bone Though it has received the name of a "needle" it woulc probably be more correctly termed a "bodkin," as being toc slender to force a passage through skins of animals—anc there is no reason to suppose that there were any contemporary textile fabrics—it was probably employed to carry threads through holes made with bone awls, such as that represented in Fig. 5.

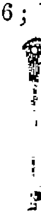


Fig. 6.

"Such needles," says Mr. Evans, "have been found in considerable numbers in the caves of the age of La Madelaine, such as Les Eyzies, Laugerie Basse, Bruniquel, and the lower cave of Massat. . . . They vary in length from $3\frac{1}{4}$ inches to 1 inch, and some have been found that shew that after they had been accidentally broken through the eye, a fresh eye was drilled. That this could readily be effected by means of a pointed flint was proved to demonstration by the late Mons. E. Lartet, who both made bone needles and bored eyes in them by means of flint tools alone." *

* *Ancient Stone Implements*, p. 461.

Fig. 7 (No. 1,929, *Cavern Journal*, and Fig. 406, Ev
is that of a well-formed bone pin, found January 3, 18
in the fourth foot-level of cave earth—the greatest dept
which it has been excavated—in immediate contact with

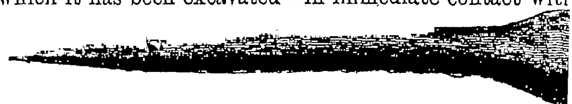


Fig. 7. $\frac{1}{1}$

unworn crown of a molar tooth of a rhinoceros. Vertic
over them there lay, in ascending order, four feet of c
earth; then the black band with its profusion of flint to
and remains of the hyæna and other cave mammals; c
this the granular stalagmitic floor, 20 inches thick,
fectly intact, and continuous in all directions; this
surmounted by the black mould; and the whole was crow
with large blocks of limestone, cemented with carbonat
lime into a firm mass, which reached the roof and al
completely separated the "Vestibule" from the rest of
cavern. The pin is well made, has a distinct head, i
immediately behind which it tapers off to a sharp point
is almost perfectly round, and has a considerable polish,
latter being in all probability the result of having been
stantly used to fasten the skin dress of its owner.

Fig. 8 (No. 2,206, *Cavern Journal*, and Fig. 404, Ev
represents a bone "harpoon," found with a flint flake a
bone apparently cut artificially, in the first or upper

Fig. 8.

foot-level of cave earth, beneath the black band, which i
turn was covered with the granular stalagmitic floor
12 to 20 inches thick, and over this again was the b
mould with its Pre-Roman objects. When dug out it
as at present, in two pieces, one almost, and the other
pletely enveloped in stalagmite. Indeed, the latter po
was regarded, and packed away with the entire "find
a small pipe or stem of stalagmite, and the discovery c

true character was made, November 28, 1868, under ci-cumstances precisely similar to those described in the cas of the "needle." Though broken, it is very nearly perfec and is barbed on one edge only.

Another "harpoon" (No. 1,970, *Cavern Journal*, and Fig 405, Evans), similarly barbed, was found with sixteen flin flakes and a flint core, in the black band, January 18, 1867 It is less perfect and has seen more service than No. 2,206

Fig. 9 (No. 2,282, *Cavern Journal*, and Fig. 403, Evans is a representation of a third bone "harpoon," which differs from those just mentioned in being barbed on two opposite sides, the barbs being also opposite, not alternate. It was found, March 18, 1867, in the second foot-level of cave earth, and over this was the usual succession of deposits found in the "vestibule." Like all bones found in the cave earth, the "harpoon" when applied to the tongue firmly adheres to it; in other words, it has the condition which, from the spot it occupied, might have been expected. The striated marks of the tool with which it was scraped into form are still distinctly visible in places. "Harpoons," both doubly and singly barbed, of precisely the same character, have been found in the Cave of La Madelaine, in the Dordogne, France, where they usually consist of reindeer horn, which was not improbably the case in the Kent's Hole specimens also. Implements of this kind have been found in numerous localities on the Continent.

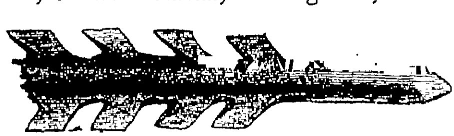


Fig. 9. ½

There was also found in the second foot-level of cave earth, in the vestibule, February 4, 1867, a canine tooth of a badger, the fang of which had been reduced to a wedge-like form, and perforated obliquely, as if for the purpose of being strung. The overlying stalagmitic floor had been broken by the early explorers; but the superintendents of the investigation now in progress have no doubt that the soil in which the tooth lay was intact, and that the specimen may be taken as an indication that the cave men of

the cave earth period occupied themselves in making
ments as well as objects of mere utility.

Through the assistance of Mr. Spence Bate, who
made a drawing
original, I an
abled to give
accompanying
of a charact
specimen of the
found in the b
of which the foll
description was
in the Comm
Ninth An
Report :—

Fig. 10. ½

Fig. 10 (No. (
Cavern Journa
presents, on the
of one-half, lines
fine kite-shaped
5·1 inches long
inches in gr
breadth, and 2 i
in greatest thick
On one side, espe
at the butt-end,
very convex; or
other it may be
to have a tenden
flatness; but as
inner face consis
two principal p
or facets slopin
opposite direc
from a transverse ridge about midway in its length
flatness is not strongly pronounced. At the butt-end
the convex face, it retains much of the original surfa
the nodule, and shews that it was made from a well-r
pebble. The rest of the surface has a somewhat ora
coloured tint, derived, no doubt, from the matrix in w
it was found. On one or two small facets near the p

however, this tint does not appear, but the true whitish colour is displayed. . . . Within the substance of the implement, and near the point, there is a small irregular quartz pebble. . . . This specimen was found on November 27, 1872, at a depth of 16 inches in the undisturbed breccia, under a block of limestone measuring $24 \times 14 \times 14$ inches, adjacent to the left wall of the 'Arcade,' 73 feet from its entrance, that is, about 160 feet from the nearest external entrance of the cavern. No animal remains or other objects of interest were found near it."*

In looking at these deposits, I find that it is possible to classify them palæontologically. For instance, in the Black Mould, or uppermost deposit, the remains of sheep occur; hence, and so far as Kent's Hole is concerned, I call that the "Ovine" or sheep deposit. The Granular Stalagmite, Black Band, and Cave Earth, contain the hyæna as master of the situation. I call these the Hyænine deposits. The teeth in the dental formula of the hyæna are comparatively very few in number. Nevertheless, on taking all the teeth we have found, the hyæna's form from 30 to 40 per cent. of the whole series, shewing that the hyæna was by far the most prevalent form. The cavern was his home; and, liking his game very high, he dragged thither piecemeal such dead animals as he found beyond the cave; there he ate them, and there he left fragments of their skeletons. The hyæna was a cave-dweller, but very few of the others were. If the hyæna had not been there the bones of the others would not have been there in all probability. Below the Cave Earth we have, omitting the few traces of lion and fox, nothing but the bear; and we call the Crystalline Stalagmite and the Breccia it covers the "Ursine" or bear deposits.

I now come to the mineral condition of the bones. Those from the ovine deposit are light, and will flo.t in water. Below the ovine bed they are heavy, and will sink in water. They have acquired a large amount of mineral matter. But whilst the bones from the hyænine and ursine deposits are thus heavy, the one series can be distinguished from the other. Those from the ursine deposits emit a metallic ring

* See *Report Brit. Assoc.*, 1873, p. 206.

when struck, which the hyænine specimens do not. Bones from either of the deposits, except the ovine or uppermost, have lost their gelatine, and will stick to the tongue when applied to it. The bones are everywhere found lying together without any approach to anatomical juxtaposition—jaws and phalanges, shin bones and skulls, all mixed up together without any sort of order. Moreover, the remains of different kinds of animals are confusedly mixed together. Many of the bones in the cave earth are invested with thin films of stalagmite. The explanation seems to be this:—They lay for a considerable period upon what was for the time the floor of the cavern, exposed to the drip from the roof; that drip caused an accretion of stalagmite over them; then an instalment of cave earth was introduced and deposited, and prevented the continuation of that accretion. Examples of this are met with at all levels in the deposit, and shew that the cave earth was introduced in small quantities and with protracted intermittences, and that there never was a time when the precipitation of the stalagmitic matter was not going on. Next, bones lying under great blocks of limestone (and some of them weigh fully 100 tons) are crushed, but the fragments lie together and in juxtaposition. The bones were crushed where they lie by the limestone block encountering them in its fall as they lay on a hard, firm, unyielding surface—which shews that Dr. Mantel was wrong when he explained some of the phenomena of Kent's Hole, discovered by MacEnery, by saying that the deposit was in a pulpy condition, and that such heavy objects as flint tools had sunk through it. (See *Jour. Archæo. Inst. Great Britain and Ireland*, vol. vii., pp. 340–41) [illegible] hen we have bones broken obliquely. These puzzled me a good deal, but ultimately convinced me that they were the work of the hyæna; and as there is nothing like experiment for such questions, I went one day to the [illegible] the hyænas in the Zoological Gardens, London, and [illegible] be introduced to the most powerful of his hyænas, and requested him, if consistent with his duty, to give "Jack" a shin bone. It was complied with, and Jack was so good as operate on the bone for my edification, when he proceeded thus:—He took the bone between and across his jaws, with one end "fair" with his lips on

one side, and the other end projecting on the other That projecting end he set on the floor, and then placing his foot on the bone, between the end and his jaws, he, by heaving and wrenching, broke it. This is what he produced, and this is one from the cavern. Are they not as like as two peas? I said to the keeper—"If quite consistent with your duty, give Jack any number of shin bones during the next week, and crib from him what you can." When I returned to the Gardens at the end of the week, I found a considerable number of bones put aside for me, and all broken after the same type, when I felt perfectly satisfied that the obliquely-broken bones so frequently found in the cavern had been operated on by hyænas. Teeth marks of the hyæna are found on many of the cavern bones; and Mr. Young of your museum shewed me to-day some Kent's Cavern bones, sent there long ago by Dr. Buckland, and on these I saw the teeth marks; and I trust that if any of you are sceptical on the point, you will go there and see them. When a hyæna got possession of a jaw he appears to have first taken off the condyles, as almost all the jaws found in the cave earth have lost their condyles. Next he removed the lower border of the jaw, leaving the upper border and the teeth intact. Then there were bones split longitudinally. Though satisfied that the infra-human animals could not do that, for information about it I went to the Zoological Gardens, and talked thus with the man who kept the lions and tigers:—"Can your most powerful lions or tigers split bones in that way?" "No." "But they like marrow. How do they get it?" "We tell the butcher to split the bones for them." By whom, then, were these bones split? Surely man must have done it. But how could man split a bone longitudinally, seeing that his most powerful tools were flint implements, such as those now before us? I set to work to see whether a man as destitute of tools as the men of the cave-earth era could not have split a bone. I observed that the articulating ends of the split bones were gone, and assumed that to have been a preliminary step in the work. I sent to the butcher to send me shin bones of ox, and they came up in considerable numbers. Then finding, not shaping, a large stone in a rockery which looked likely to do very well

for an anvil, I dealt the bone a blow on it, and thereby did nothing towards solving the problem. I then put the bone on the anvil, with one end projecting, and holding it firmly in that position, I took another stone as an extemporised sledge, and dealing it a heavy blow, broke the end of the bone off short, and then broke off the other in the same way. Then it became clear that if our savage friend wanted the marrow, he might take a bit of stick and pick it out, and need not be at the pains of *splitting* the bone. I went then to a tree in my garden, and broke down a branch, stripped it of the twigs, and inserted in the cavity the smaller end of the branch, and then using the stick and bone united, as a pavior uses his "rammer," at length succeeded in splitting the bone. I don't say that that was the way they *were* split, but I do say that man could have split them thus; and that his object was not to get the marrow, but "laths" of bone to make bone implements, such as we know he used.

With reference to the *black band* near the doorway, I believe that to have been more especially the dwelling-place of the ancient cave men. They selected the spot because it was one of the driest parts of the cavern, because it was conveniently near the entrance, and because the daylight entering through the entrance was available. I have sat on the spot, and written letters and read small print without making use of artifical light. Indeed, there is, in my opinion, a good deal of error about the defectiveness of light in caverns. Thus, my friend Mr. Prestwich speaks of objects found in Brixham Cavern "30 and 50 feet (and in one case as many as 74 feet) from the nearest external entrance, where, he says, little if any light could penetrate." (*Phil. Trans.*, vol. clxiii., pt. 2, p. 564.) To test this I went into Kent's Cavern, and selected a spot where the light did not enter under more favourable circumstances than in Brixham Cavern, and sat down 90 feet from the entrance. I scattered about a number of keys, and coppers, and other small objects, and then wrote a passage in a paper, using the daylight which reached me only. (See *Trans. Devon. Assoc.*, vol. vi., pp. 850-51.) I then gathered up my keys, coppers, &c., and found that I had lost nothing whatever. Our poor fellows in the

Crimea would have been glad of such a home as Kent's Cavern, for it is quite a warm place. I may state in passing, that if you get into such a cavern, one reading of the thermometer suffices for the whole year. The influence of day and night, of summer and winter is not felt there. The temperature is unvarying, and is the mean annual temperature of the district. As nearly as possible, $51\frac{1}{2}^{\circ}$ at Torquay. On the area of the black ...and, I presume, these men lived; there they made their ...res; there they chipped their flints into implements. Hence the large number we found there; hence the bone ...ols we found there, the burnt bones, and the prodigious ...nount of charcoal. A friend of mine said, "If you light ... fire there, you will send the people to death." To test ...is I got six faggots of wood, piled them one on another, ...d set fire to them. A clergyman, my son, myself, and ...e workmen sat round until the whole were consumed, ...d we not only live to tell the tale, but were in no way ...convenienced by the smoke.

There are several questions you would like to ask me, ... doubt; but the present lecture is one of four or five on ...e same subject. I have put before you the facts of the ...vern, and you can draw the inferences. It may be as ...ll, however, to state just one or two things. The black ...uld, the overlying or most recent of the deposits, must ... worth, as a minimum, two thousand years, and may be ...rth twenty thousand; and everything below it is older. ...en that granular stalagmite was formed slowly. There ... inscriptions on it probably not more than one-eighth of ... inch deep when they were cut. The 250 years that ...e elapsed since then have produced a mere film, which ... not only failed to obliterate them, but is not more than ...-twentieth of an inch thick. That, were we sure that ... rate of accretion had been uniform, would give 5,000 ...rs for an inch of the stalagmite. I don't say you must ...e that as a chronometer, but I do say to those who ...ct to it (and I do not say that I accept it myself), "Tell ... why you object." Suppose that it was formed fifty ...es faster, still the stalagmite is but one unit in the scale, ... fifty times quicker than the present rate will lead you ... result at which our fathers would have stood aghast

Below that comes the cave earth, and below that again the prodigiously thicker crystalline stalagmite, and still below that the breccia.

It appears to me perfectly safe to believe that if the hyæna had been in the country during the ursine or breccia era, his remains would be found in the breccia. Britain must have been continental after this earliest of the cavern periods, during which man, but not the hyæna, was living in Devonshire. If my conclusion is a sound one, then the earliest Devonshire men known to us saw Britain an island, as we see it. Their descendants or successors saw it subsequently continental. Their successors again saw it an island. Indeed, unless the men of the ursine era had the means of navigation, they must have reached Britain during the earliest of its Pleistocene continental periods. In short, they were either inter-glacial or pre-glacial.

Now comes another question. Was the primal man, supposing we are at liberty to use such a phrase, a savage or a civilised being? I cannot answer this; but I know that the farther I have pursued man into antiquity, the ruder he has turned out to be. I don't say that he has not descended from ancestors vastly superior to himself; but if so, the ancestors are to be sought in a still more remote antiquity. Further, if there were such men of higher culture and higher attainments than those in the ursine period, why is it that we do not in the older deposits find a greater number and a greater variety of more highly finished tools? When they are forthcoming, science will receive them gratefully; and if they are never produced, I trust those who have led us to expect them will endeavour to account for the fact. I have been endeavouring to deal with the evidence of the antiquity of man? No—the evidence of the *antiquity of man in Devonshire*, which, in all probability, was far away from the cradle of the human race. And hence you perceive you must add to the time I have endeavoured to sketch all that was required for the multiplication of the population in the original human area, and their consequent migrations thence to Great Britain. That I believe will take you back to an antiquity very far beyond that of the men who used those flint implements found in the breccia of Kent's Cavern.

FERMENTATION.

In a book with which we are all familiar, amid other wise utterances, this one occurs: "Cast thy bread upon the waters; for thou shalt find it after many days." In more senses than one this precept is illustrated by my presence here to-night. Firstly, in a general sense, I stand indebted, norally and intellectually, to the poets, historians, and philosophers of Scotland. Secondly, in a special sense, it so happens that one of the first rootlets of my scientific life derived ts nutriment from this city of Glasgow. In early youth it vas my ambition to qualify myself for the profession of a ivil engineer, and as I grew up one of my aids toward the ttainment of this object was the study of a periodical then ublished in Glasgow, and called *The Practical Mechanic's nd Engineer's Magazine.* In that journal I read, with an iterest unfelt before, a series of essays on various departents of science—on anatomy and physiology, on geology, 1 mechanics, on arithmetic, and on natural philosophy and emistry. Biography and history were also included, while detached articles various collateral subjects were discussed, ch, for example, as the difference between Newton and ibnitz as to the measure of moving force. It was there at I first learned what Leslie had done in Edinburgh, and at Davy had done in the Royal Institution. And I can w call to mind the day and hour when the yearning to ssess such apparatus as Leslie and Davy possessed, and to titute with it such inquiries as they had instituted, rose a kind of prophetic strength within me—prophetic, for it

has come to pass that my own studies as a scientifi
have been in great part pursued in that particular d
which had been enriched by the discoveries of Leslie;
the very instruments used by Davy, and which I firs
figured in the pages of the journal just mentioned, a
identical and familiar instruments with which my le
in London are now illustrated.

Another point brought more or less home to me in
early days was the injury inflicted on the learner b
scientific exposition. It does more than the negative de
of withholding instruction. It daunts the young mind
saps the motive power of self-reliance. This I had
rienced; and the essays referred to had this special
for me, that they not only instructed me, but gave me
in my own capacity to be instructed. Since those d
have written books myself, and in doing so have tri
remember, and to act on the remembrance, that the la
spent in logically ordering one's thoughts, and in s
what one has to say clearly and correctly, is labour
bestowed.

The position assumed at the outset has, I think, been
made good. Glasgow in my case cast its bread upon
waters, and lo! it has returned after many days. O
nutritive value of the return it is not for me to speak
it may well have been soured by fortuitous ferments, n
by the world's tainted atmosphere with the first pure le
derived from the pages of *The Practical Mechanic's*
Engineer's Magazine.

The figure of speech here employed will become
intelligible as we proceed; for it is my desire and inter
to spend the coming hour in speaking to you about *ferm*
not in a metaphorical, but in a real sense. Proper treat
is, I am persuaded, the only thing needed to make the
ject both pleasant and profitable to you. For our knowl
of fermentation, and of the ground it covers, has augme
surprisingly of late, while every fresh accession to that kn
ledge strengthens the hope that its final issues will be o
calculable advantage to mankind.

One of the most remarkable characteristics of the ag

which we live, is its desire and tendency to connect itself organically with preceding ages—to ascertain how the state of things that now is came to be what it is. And the more earnestly and profoundly this problem is studied, the more clearly comes into view the vast and varied debt which the world of to-day owes to that fore-world, in which man by skill, valour, and well-directed strength first replenished and subdued the earth. Our pre-historic fathers may have been savages, but they were clever and observant ones. They founded agriculture by the discovery and development of seeds whose origin is now unknown. They tamed and harnessed their animal antagonists, and sent them down to us as ministers, instead of rivals in the fight for life. Later on, when the claims of luxury added themselves to those of necessity, we find the same spirit of invention at work. We have no historic account of the first brewer, but we glean from history that his art was practised, and its produce relished, more than two thousand years ago. Theophrastus, who was born nearly four hundred years before Christ, described beer as *the wine of barley*. It is extremely difficult to preserve beer in a hot country, still, Egypt was the land in which it was first brewed, the desire of man to quench his thirst with this exhilarating beverage overcoming all the obstacles which a hot climate threw in the way of its manufacture.

Our remote ancestors had also learned by experience that wine maketh glad the heart of man. Noah, we are informed, planted a vineyard, drank of the wine, and experienced the consequences. But, though wine and beer possess so old a history, a very few years ago no man knew the secret of their formation. Indeed, it might be said that until the present year no thorough and scientific account was ever given of the agencies which come into play in the manufacture of beer, of the conditions necessary to its health, and of the maladies and vicissitudes to which it is subject. Hitherto the art and practice of the brewer have resembled those of the physician, both being founded on empirical observation. By this is meant the observation of facts apart from the principles which explain them, and which give the mind an intelligent mastery over them. The brewer learnt from

long experience the conditions, not the reasons of success. But he had to contend, and he has still to contend, against unexplained perplexities. Over and over again his care has been rendered nugatory; his beer has fallen into acidity or rottenness, and disastrous losses have been sustained, of which he has been unable to assign the cause. It is the hidden enemies against which the physician and the brewer have hitherto contended, that recent researches are dragging into the light of day, thus preparing the way for their final extermination.

Let us glance for a moment at the outward and visible signs of fermentation. A few weeks ago I paid a visit to a private still in a Swiss chalet; and this is what I saw. In the peasant's bedroom was a cask with a very large bung-hole carefully closed. The cask contained cherries which had lain in it for fourteen days. It was not entirely filled with the fruit, an air-space being left above the cherries when they were put in. I had the bung removed, and a small lamp dipped into this space. Its flame was instantly extinguished. The oxygen of the air had entirely disappeared, its place being taken by carbonic acid gas.* I tasted the cherries: they were very sour, though when put into the cask they were sweet. The cherries and the liquid associated with them were then placed in a copper boiler, to which a copper head was closely fitted. From the head proceeded a copper-tube which passed straight through a vessel of cold water, and issued at the other side. Under the open end of the tube was placed a bottle to receive the spirit distilled. The flame of small wood splinters being applied to the boiler, after a time vapour rose into the head, passed through the tube, was condensed by the cold of the water, and fell in a liquid fillet into the bottle. On being tasted, it proved to be that fiery and intoxicating spirit known in commerce as Kirsch or Kirschwasser.

The cherries, it should be remembered, were here left to themselves, no ferment of any kind being added to them. In this respect what has been said of the cherry applies also

* The gas which is exhaled from the lungs after the oxygen of the air has done its duty in purifying the blood, the same also which effervesces from soda water and champagne.

to the grape. At the vintage the fruit of the vine is placed in proper vessels, and abandoned to its own action. It ferments, producing carbonic acid; its sweetness disappears, and at the end of a certain time the unintoxicating grape-juice is converted into intoxicating wine. Here, as in the case of the cherries, the fermentation is spontaneous—in what sense spontaneous will appear more clearly by-and-by.

It is needless for me to tell a Glasgow audience that the beer-brewer does not set to work in this way. In the first place the brewer deals not with the juice of fruits, but with the juice of barley. The barley having been steeped for a sufficient time in water, it is drained, and subjected to a temperature sufficient to cause the moist grain to germinate, after which it is completely dried upon a kiln. It then receives the name of malt. The malt is crisp to the teeth, and decidedly sweeter to the taste than the original barley. It is ground, mashed up in warm water, then boiled with hops until all the soluble portions have been extracted, the infusion thus produced being called the *wort*. This is drawn off, and cooled as rapidly as possible; then, instead of abandoning the infusion, as the wine-maker does, to its own action, the brewer mixes yeast with his wort, and places it in vessels, each with only one aperture open to the air. Soon after the addition of the yeast, a brownish froth, which is really new yeast, issues from the aperture, and falls like a cataract into troughs prepared to receive it. This frothing and foaming of the wort is a proof that the fermentation is active.

Whence comes the yeast which issues so copiously from the fermenting tub? What is this yeast, and how did the brewer become in the first instance possessed of it? Examine its quantity before and after fermentation. The brewer introduces, say 10 cwts. of yeast; he collects 40, or it may be 50 cwts. The yeast has, therefore, augmented from four to five fold during the fermentation. Shall we conclude that this additional yeast has been spontaneously generated by the wort? Are we not rather reminded of that seed which fell into good ground, and brought forth fruit, some thirty-fold, some sixty-fold, some an hundred-fold? On examination this notion of organic growth turns out to be more than

a mere surmise. In the year 1680, when the microscope was still in its infancy, Leeuwenhoek turned the instrument upon this substance, and found it composed of minute globules suspended in a liquid. Thus knowledge rested until 1835, when Cagniard de la Tour in France, and Schwann in Germany, independently, but animated by a common thought, turned microscopes of improved definition and heightened powers upon yeast, and found it budding and sprouting before their eyes. The augmentation of the yeast alluded to above was thus proved to arise from the growth of a minute plant, now called *Torula* (or *Saccharomyces*) *Cerevisiœ*. Spontaneous generation is therefore out of the question. The brewer deliberately sows the yeast-plant, which grows and multiplies in the wort as its proper soil. This discovery marks an epoch in the history of fermentation.

But where did the brewer find his yeast? The reply to this question is similar to that which must be given if it were asked where the brewer found his barley. He has received the seeds of both of them from preceding generations. Could we connect without solution of continuity the present with the past, we should probably be able to trace back the yeast employed by my friend Sir Fowell Buxton to-day, to that employed by some Egyptian brewer two thousand years ago. But you may urge that there must have been a time when the first yeast cell was generated. Granted—exactly as there was a time when the first barley-corn was generated. Let not the delusion lay hold of you, that a living thing is easily generated, because it is small. Both the yeast-plant and the barley-plant lose themselves in the dim twilight of antiquity, and in this our day there is no more proof of the spontaneous generation of the one, than there is of the spontaneous generation of the other.

I stated a moment ago that the fermentation of grape-juice was spontaneous; but I was careful to add, "in what sense spontaneous will appear more clearly by-and-by." Now this is the sense meant. The wine-maker does not, like the brewer and distiller, deliberately introduce either yeast, or any equivalent of yeast, into his vats; he does not consciously sow in them any plant, or the germ of any plant; indeed, he has been hitherto in ignorance whether plants or

germs of any kind have had anything to do with his operations. Still, when the fermented grape-juice is examined, the living *Torula* concerned in alcoholic fermentation never fails to make its appearance. How is this? If no living germ has been introduced into the wine vat, whence comes the life so invariably developed there?

You may be disposed to reply with Turpin and others, that in virtue of its own inherent powers, the grape-juice, when brought into contact with the vivifying atmospheric oxygen, runs spontaneously and of its own accord into these low forms of life. I have not the slightest objection to this explanation, provided proper evidence can be adduced in support of it. But the evidence adduced in its favour, as far as I am acquainted with it, snaps asunder under the least strain of scientific criticism. It is, as far as I can see, the evidence of men, who, however keen and clever as *observers*, are not rigidly trained *experimenters*. These alone are aware of the precautions necessary in investigations of this delicate kind. In reference, then, to the life of the wine vat, what is the decision of experiment when carried out by competent men? Let a quantity of the clear, filtered "must" of the grape be so boiled as to destroy such germs as it may have contracted from the air or otherwise. In contact with germless air the uncontaminated must never ferments. All the materials for spontaneous generation are there, but so long as there is no seed sown there is no life developed, and no sign of that fermentation which is the concomitant of life. Nor need you resort to a boiled liquid. The grape is sealed by its own skin against contamination from without. By an ingenious device Pasteur has extracted from the interior of the grape its pure juice, and proved that in contact with pure air it never acquires the power to ferment itself, nor to produce fermentation in other liquids.* It is not, therefore, in the interior of the grape that the origin of the life observed in the vat is to be sought.

* The liquids of the healthy animal body are also sealed from external contamination. Pure blood, for example, drawn with due precautions from the veins, will never ferment or putrefy in contact with pure air.

What then is its true origin? This is Pasteur's answ which his well-proved accuracy renders worthy of all con dence. At the time of the vintage microscopic particles observed adherent, both to the outer surface of the gra and of the twigs which support the grape. Brush the particles into a capsule of pure water. It is rendered turl by the dust. Examined by a microscope some of the minute particles are seen to present the appearance organised cells. Instead of receiving them in water, l them be brushed into the pure inert juice of the gra Forty-eight hours after this is done, our familiar *Torula* observed budding and sprouting, the growth of the pla being accompanied by all the other signs of active ferment tion. What is the inference to be drawn from this expe ment? Obviously that the particles adherent to the extern surface of the grape include the germs of that life whic after they have been sown in the juice, appears in suc profusion. Wine is sometimes objected to on the groun that fermentation is "artificial;" but we notice here th responsibility of nature. The ferment of the grape cling like a parasite to the surface of the grape, and the art of th wine-maker from time immemorial has consisted in bringin —and it may be added, ignorantly bringing—two thing thus closely associated by nature into actual contact wit each other. For thousands of years, what has been don consciously by the brewer, has been done unconsciously b the wine-grower. The one has sown his leaven just as muc as the other.

Nor is it necessary to impregnate the beer-wort with yeas to provoke fermentation. Abandoned to the contact of ou common air, it sooner or later ferments; but the chances are that the produce of that fermentation, instead of being agreeable, would be disgusting to the taste. By a rare accident we might get the true alcoholic fermentation, but the odds against obtaining it would be enormous. Pure air acting upon a lifeless liquid will never provoke fermentation; but our ordinary air is the vehicle of numberless germs which act as ferments when they fall into appropriate infusions. Some of them produce acidity, some putrefaction. The germs of our yeast-plant are also in the air; but so sparingly

distributed that an infusion like beer-wort, exposed to the air, is almost sure to be taken possession of by foreign organisms. In fact the maladies of beer are wholly due to the admixture of these objectionable ferments, whose forms and modes of nutrition differ materially from those of the true leaven.

Working in an atmosphere charged with the germs of these organisms, you can understand how easy it is to fall into error in studying the action of any one of them. Indeed it is only the most accomplished experimenter, who, moreover, avails himself of every means of checking his conclusions, that can walk without tripping through this land of pitfalls. Such a man is the French chemist Pasteur. He has taught us how to separate the commingled ferments of our air, and to study their pure individual action. Guided by him, let us fix our attention more particularly upon the growth and action of the true yeast-plant under different conditions. Let it be sown in a fermentable liquid, which is supplied with plenty of pure air. The plant will flourish in the aërated infusion, and produce large quantities of carbonic acid gas—a compound, as you know, of carbon and oxygen. The oxygen thus consumed by the plant is the free oxygen of the air, which we suppose to be abundantly supplied to the liquid. The action is so far similar to the respiration of animals, which inspire oxygen and expire carbonic acid. If we examine the liquid even when the vigour of the plant has reached its maximum, we hardly find in it a trace of alcohol. The yeast has grown and flourished, but it has almost ceased to act as a ferment. And could every individual yeast cell seize, without any impediment, free oxygen from the surroundiug liquid, it is certain that it would cease to act as a ferment altogether.

What, then, are the conditions under which the yeast-plant must be placed so that it may display its characteristic quality? Reflection on the facts already referred to suggests a reply, and rigid experiment confirms the suggestion. Consider the Alpine cherries in their closed vessel. Consider the beer in its barrel, with a single small aperture open to the air, through which it is observed not to imbibe oxygen, but to pour forth carbonic acid. Whence come the volumes

of oxygen necessary to the production of this latter gas? The small quantity of atmospheric air dissolved in the wort and overlying it would be totally incompetent to supply the necessary oxygen. In no other way can the yeast-plant obtain the gas necessary for its respiration than by wrenching it from surrounding substances in which the oxygen exists, not free, but in a state of combination. It decomposes the sugar of the solution in which it grows, produces heat, breathes forth carbonic acid gas, and one of the liquid products of the decomposition is our familiar alcohol. The act of fermentation, then, is a result of the effort of the little plant to maintain its respiration by means of combined oxygen, when its supply of free oxygen is cut off. As defined by Pasteur, fermentation is *life without air.*

But here the knowledge of that thorough investigator comes to our aid to warn us against errors which have been committed over and over again. It is not all yeast cells that can thus live without air and provoke fermentation. They must be young cells which have caught their vegetative vigour from contact with free oxygen. But once possessed of this vigour the yeast may be transplanted into a saccharine infusion absolutely purged of air, where it will continue to live at the expense of the oxygen, carbon, and other constituents of the infusion. Under these new conditions its life *as a plant* will be by no means so vigorous as when it had a supply of free oxygen, but its action *as a ferment* will be indefinitely greater.

Does the yeast-plant stand alone in its power of provoking alcoholic fermentation? It would be singular if amid the multitude of low vegetable forms no other could be found capable of acting in a similar way. And here again we have occasion to marvel at that sagacity of observation among the ancients to which we owe so vast a debt. Not only did they discover the alcoholic ferment of yeast, but they had to exercise a wise selection in picking it out from others, and giving it special prominence. Place an old boot in a moist place, or expose common paste or a pot of jam to the air; it soon becomes coated with a blue-green mould, which is nothing else than the fructification of a little plant called *Penicillium glaucum.* Do not imagine that the mould has sprung spon-

taneously from boot, or paste, or jam; its germs, which are abundant in the air, have been sown, and have germinated, in as legal and legitimate a way as thistle-seeds wafted by the wind to a proper soil. Let the minute spores of *Penicillium* be sown in a fermentable liquid, which has been previously so boiled as to kill all other spores or seeds which it may contain; let pure air have free access to the mixture; the *Penicillium* will grow rapidly, striking long filaments into the liquid, and fructifying at its surface. Test the infusion at various stages of the plant's growth, you will never find in it a trace of alcohol. But forcibly submerge the little plant, push it down deep into the liquid, where the quantity of free oxygen that can reach it is insufficient for its needs, it immediately begins to act as a ferment, supplying itself with oxygen by the decomposition of the sugar, and producing alcohol as one of the results of the decomposition. Many other low microscopic plants act in a similar manner. In aërated liquids they flourish without any production of alcohol, but cut off from free oxygen they act as ferments, producing alcohol exactly as the real alcoholic leaven produces it, only less copiously. For the right apprehension of all these facts we are indebted to Pasteur.

In the cases hitherto considered, the fermentation is proved to be the invariable correlative of *life*, being produced by organisms foreign to the fermentable substance. But the substance itself may also have within it, to some extent, the motive power of fermentation. The yeast-plant, as we have learned, is an assemblage of living cells; but so at bottom, as shown by Schleiden and Schwann, are all living organisms. Cherries, apples, peaches, pears, plums, and grapes, for example, are composed of cells, each of which is a living unit. And here I have to direct your attention to a point of extreme interest. In 1821 the celebrated French chemist Bérard established the important fact that all ripening fruit, exposed to the free atmosphere, absorbed the oxygen of the atmosphere, and liberated an approximately equal volume of carbonic acid. He also found that when ripe fruits were placed in a confined atmosphere, the oxygen of the atmosphere was first absorbed, and an equal volume of carbonic acid given out. But the process did not end here. After

the oxygen had vanished, carbonic acid, in considerable quantities, continued to be exhaled by the fruits, which at the same time lost a portion of their sugar, becoming more acid to the taste, though the absolute quantity of acid was not augmented. This was an observation of capital importance, and Bérard had the sagacity to remark that the process might be regarded as a kind of fermentation.

Thus the living cells of fruits can absorb oxygen and breathe out carbonic acid, exactly like the living cells of the leaven of beer. Supposing the access of oxygen suddenly cut off, will the living fruit-cells as suddenly die, or will they continue to live as yeast lives, by extracting oxygen from the saccharine juices round them? This is a question of extreme theoretic significance. It was first answered affirmatively by the able and conclusive experiments of Lechartier and Bellamy, and the answer was subsequently confirmed and explained by the experiments and the reasoning of Pasteur. Bérard only showed the absorption of oxygen and the production of carbonic acid; Lechartier and Bellamy proved the production of alcohol, thus completing the evidence that it was a case of real fermentation, though the common alcoholic ferment was absent. So full was Pasteur of the idea that the cells of a fruit would continue to live at the expense of the sugar of the fruit, that once in his laboratory, while conversing on these subjects with M. Dumas, he exclaimed, "I will wager that if a grape be plunged into an atmosphere of carbonic acid, it will produce alcohol and carbonic acid by the continued life of its own cells—that they will act for a time like the cells of the true alcoholic leaven." He made the experiment, and found the result to be what he had foreseen. He then extended the inquiry. Placing under a bell-jar twenty-four plums, he filled the jar with carbonic acid gas; beside it he placed twenty-four similar plums uncovered. At the end of eight days he removed the plums from the jar, and compared them with the others. The difference was extraordinary. The uncovered fruits had become soft, watery, and very sweet; the others were firm and hard, their fleshy portions being not at all watery. They had, moreover, lost a considerable quantity of their sugar. They were afterwards bruised, and the juice was distilled.

It yielded six and a half grammes of alcohol, or one per cent. of the total weight of the plums. Neither in these plums, nor in the grapes first experimented on by Pasteur, could any trace of the ordinary alcoholic leaven be found. As previously proved by Lechartier and Bellamy, the fermentation was the work of the living cells of the fruit itself, after air had been denied to them. When, moreover, the cells were destroyed by bruising, no fermentation ensued. The fermentation was the correlative of a vital act, and it ceased when life was extinguished.

Lüdersdorf was the first to show by this method that yeast acted not, as Liebig had assumed, in virtue of its *organic*, but in virtue of its *organised* character. He destroyed the cells of yeast by rubbing them on a ground glass plate, and found that with the destruction of the organism, though its chemical constituents remained, the power to act as a ferment totally disappeared.

One word more in reference to Liebig may find a place here. To the philosophic chemist thoughtfully pondering these phenomena, familiar with the conception of molecular motion, and the changes produced by the interactions of purely chemical forces, nothing could be more natural than to see in the process of fermentation a simple illustration of molecular instability, the ferment propagating to surrounding molecular groups the overthrow of its own tottering combinations. Broadly considered, indeed, there is a certain amount of truth in this theory; but Liebig, who propounded it, missed the very kernel of the phenomena when he overlooked or contemned the part played in fermentation by microscopic life. He looked at the matter too little with the eye of the body, and too much with the spiritual eye. He practically neglected the microscope, and was unmoved by the knowledge which its revelations would have poured in upon his mind. His hypothesis, as I have said, was natural—nay, it was a striking illustration of Liebig's power to penetrate and unveil molecular actions; but it was an error, and as such has proved an *ignis fatuus* instead of a *pharos* to some of his followers.

I have said that our air is full of the germs of ferments

differing from the alcoholic leaven, and sometimes seriousl; interfering with the latter. They are the weeds of thi microscopic garden which often overshadow and choke th flowers. Let us take an illustrative case. Expose boilec milk to the air. It will cool, and then turn sour, separatin; like blood into clot and serum. Place a drop of this sou: milk under a powerful microscope and watch it closely. Yo see the minute butter-globules animated by that curiou quivering motion called the Brownian motion.* But le not this attract your attention too much, for it is anothe motion that we have now to seek. Here and there you observe a greater disturbance than ordinary among the globules; keep your eye upon the place of tumult, and you will probably see emerging from it a long eel-like organism, tossing the globules aside and wriggling more or less rapidly across the field of the microscope. Familiar with one sample of this organism, which from its motions receives the name of vibrio, you soon detect numbers of them. It is these organisms, and other analogous though apparently motionless ones, which by decomposing the milk render it sour and putrid. They are the lactic and putrid ferments, as the yeast-plant is the alcoholic ferment of sugar. Keep them and their germs out of your milk, and it will continue sweet. But milk may become putrid without becoming sour. Examine such putrid milk microscopically, and you find it swarming with shorter organisms, sometimes associated with the vibrios, sometimes alone, and often manifesting a wonderful alacrity of motion. Keep these organisms and their germs out of your milk, and it will never putrefy. Expose a mutton-chop to the air and keep it moist; in summer weather it soon stinks. Place a drop of the juice of the fetid chop under a powerful microscope; it is seen swarming with organisms resembling those in the putrid milk. These organisms, which receive the common name of bacteria,† are the agents of all putrefaction. Keep them and their germs from your meat, and it will remain for ever sweet. Thus we begin to see that within the world of life to which we our-

* Which I am inclined to regard as an effect of surface tension.

† Doubtless organisms exhibiting grave specific differences are grouped together under this common name.

selves belong, there is another living world requiring the microscope for its discernment, but which, nevertheless, has the most important bearing on the welfare of the higher life-world.

And now let us reason together as regards the origin of these bacteria. A granular powder is placed in your hands, and you are asked to state what it is. You examine it, and have, or have not, reason to suspect that seeds of some kind are mixed up in it. But you prepare a bed in your garden, sow in it the powder, and soon after find a mixed crop of docks and thistles sprouting from your bed. Until this powder was sown neither docks nor thistles ever made their appearance in your garden. You repeat the experiment once, twice, ten times, fifty times. From fifty different beds after the sowing of the powder you obtain the same crop. What will be your response to the question proposed to you? "I am not in a condition," you would say, "to affirm that every grain of the powder is a dock-seed or a thistle-seed; but I am in a condition to affirm that both dock and thistle seeds form, at all events, part of the powder." Supposing a succession of such powders to be placed in your hands with grains becoming gradually smaller, until they dwindle to the size of impalpable dust particles; assuming that you treat them all in the same way, and that from every one of them in a few days you obtain a definite crop—it may be clover, it may be mustard, it may be mignonette, it may be a plant more minute than any of these, the smallness of the particles, or of the plants that spring from them, does not affect the validity of the conclusion. Without a shadow of misgiving you would conclude that the powder must have contained the seeds or germs of the life observed. There is not in the range of physical science an experiment more conclusive nor an inference safer than this one.

Supposing the powder to be light enough to float in the air, and that you are enabled to see it there just as plainly as you saw the heavier powder in the palm of your hand. If the dust sown by the air instead of by the hand produce a definite living crop, with the same logical rigour you would conclude that the germs of this crop must be mixed with the dust. To take an illustration: the spores of the little plant

Penicillium glaucum, to which I have already referred, light enough to float in the air. A cut apple, a pear tomato, a slice of vegetable marrow, or, as already mention an old moist boot, a dish of paste, or a pot of jam, constitu a proper soil for the *Penicillium*. Now, if it could be prov that the dust of the air when sown in this soil produces t plant, while, wanting the dust, neither the air nor the so nor both together, can produce it, it would be obviously ju as certain in this case that the floating dust contains t germs of *Penicillium* as that the powders sown in yo garden contained the germs of the plants which sprung fro them.

But how is the floating dust to be rendered visible? this way. Build a little chamber and provide it with door, windows, and window-shutters. Let an aperture made in one of the shutters, through which a sunbeam ca pass. Close the door and windows so that no light sha enter save through the hole in the shutter. The track the sunbeam is at first perfectly plain and vivid in the a of the room. If all disturbance of the air of the chamber b avoided, the luminous track will become fainter and fainte until at last it disappears absolutely, and no trace of th beam is to be seen. What rendered the beam visible a first? The floating dust of the air, which, thus illuminate and observed, is as palpable to sense as any dust or powde placed on the palm of the hand. In the still air the dus gradually sinks to the floor, or sticks to the walls and ceiling until finally, by this self-cleansing process, the air is entirel freed from mechanically suspended matter.

Thus far, I think, we have made our footing sure. Le us proceed. Chop up a beefsteak and allow it to remain for two or three hours just covered with warm water; you thus extract the juice of the beef in a concentrated form By properly boiling the liquid and filtering it you can obtain from it a perfectly transparent beef-tea. Expose a number of vessels containing this tea to the moteless air of your chamber; and expose a number of similar vessels containing precisely the same liquid to the dust-laden air. In three days every one of the latter stinks, and examined with the microscope every one of them is found swarming with

the bacteria of putrefaction. After three months, or three years, the beef-tea within the chamber is found in every case as sweet and clear, and as free from bacteria as it was at the moment when it was first put in. There is absolutely no difference between the air within and that without, save that the one is dustless and the other dust-laden. Clinch the experiment thus: Open the door of your chamber and allow the dust to enter it. In three days afterwards you have every vessel within the chamber swarming with bacteria, and in a state of active putrefaction. Here, also, the inference is quite as certain as in the case of the powder sown in your garden. Multiply your proofs by building fifty chambers instead of one, and by employing every imaginable infusion of wild animals and tame; of flesh, fish, fowl, and viscera; of vegetables of the most various kinds. If in all these cases you find the dust infallibly producing its crop of bacteria, while neither the dustless air nor the nutritive infusion, nor both together, are ever able to produce this crop, your conclusion is simply irresistible that the dust of the air contains the germs of the crop which has appeared in your infusions. I repeat there is no inference of experimental science more certain than this one. In the presence of such facts, to use the words of a paper lately published in the "Philosophical Transactions," it would be simply monstrous to affirm that these swarming crops of bacteria are spontaneously generated.

Is there, then, no experimental proof of spontaneous generation? I answer without hesitation, *none!* But to doubt the experimental proof of a fact, and to deny its possibility, are two different things, though some writers confuse matters by making them synonymous. In fact, this doctrine of spontaneous generation, in one form or another, falls in with the theoretic beliefs of some of the foremost workers of this age; but it is exactly these men who have the penetration to see, and the honesty to expose, the weakness of the evidence adduced in its support.

And here observe how these discoveries tally with the common practices of life. Heat kills the bacteria, cold numbs them. When my housekeeper has pheasants in

charge which she wishes to keep sweet, but which threaten to give way, she partially cooks the birds, kills the infant bacteria, and thus postpones the evil day. By boiling her milk she also extends its period of sweetness. Some weeks ago in the Alps I made a few experiments on the influence of cold upon ants. Though the sun was strong, patches of snow still maintained themselves on the mountain slopes. The ants were found in the warm grass and on the warm rocks adjacent. Transferred to the snow, the rapidity of their paralysis was surprising. In a few seconds a vigorous ant, after a few languid struggles, would wholly lose its power of locomotion, and lie practically dead upon the snow. Transferred to the warm rock, it would revive, to be again smitten with death-like numbness when retransferred to the snow. What is true of the ant is specially true of our bacteria. Their active life is suspended by cold, and with it their power of producing or continuing putrefaction. This is the whole philosophy of the preservation of meat by cold. The fishmonger, for example, when he surrounds his very assailable wares by lumps of ice, stays the process of putrefaction by reducing to numbness and inaction the organisms which produce it, and in the absence of which his fish would remain sweet and sound. It is the astonishing activity into which these bacteria are pushed by warmth that renders a single summer's day sometimes so disastrous to the great butchers of London and Glasgow. The bodies of guides lost in the crevasses of Alpine glaciers have come to the surface forty years after their interment, without the flesh showing any sign of putrefaction. But the most astonishing case of this kind is that of the hairy elephant of Siberia which was found encased in ice. It had been buried for ages, but when laid bare its flesh was sweet, and for some time afforded copious nutriment to the wild beasts which fed upon it.

Beer is assailable by all the organisms here referred to, some of which produce acetic, some lactic, and some butyric acid, while yeast is open to attack from the bacteria of putrefaction. In relation to the particular beverage the brewer wishes to produce, these foreign ferments have been properly called *ferments of disease*. The cells of the true leaven are globules, usually somewhat elongated. The other

organisms are more or less rod-like or eel-like in shape, some of them being beaded so as to resemble necklaces. Each of these organisms produces a fermentation and a flavour peculiar to itself. Keep them out of your beer, and it remains for ever unaltered. Never without them will your beer contract disease. But their germs are in the air, in the vessels employed in the brewery; even in the yeast used to impregnate the wort. Consciously or unconsciously, the art of the brewer is directed against them. His aim is to paralyse if he cannot annihilate them.

For beer, moreover, the question of temperature is one of supreme importance; indeed the recognised influence of temperature is causing on the continent of Europe a complete revolution in the manufacture of beer. When I was a student in Berlin, in 1851, there were certain places specially devoted to the sale of Bavarian beer, which was then making its way into public favour. This beer is prepared by what is called the process of *low fermentation;* the name being given partly because the yeast of the beer, instead of rising to the top and issuing through the bunghole, falls to the bottom of the cask; but partly, also, because it is produced at a low temperature. The other and older process, called *high fermentation*, is far more handy, expeditious, and cheap. In high fermentation eight days suffice for the production of the beer; in low fermentation, ten, fifteen, even twenty days are found necessary. Vast quantities of ice, moreover, are consumed in the process of low fermentation. In the single brewery of Dreher, of Vienna, a hundred million pounds of ice are consumed annually in cooling the wort and beer. Notwithstanding these obvious and weighty drawbacks, the low fermentation is rapidly displacing the high upon the Continent. Here are some statistics which show the number of breweries of both kinds existing in Bohemia in 1860, 1865, and 1870:

	1860.	1865.	1870.
High Fermentation . .	281	8	18
Low Fermentation . .	135	459	831

Thus in ten years the number of high-fermentation breweries fell from 281 to 18, while the number of low-fermentation breweries rose from 135 to 831. The sole

reason for this vast change—a change which involves a greater expenditure of time, labour, and money—is the additional command which it gives the brewer over the fortuitous ferments of disease. These ferments, which, it is to be remembered, are living organisms, have their activity suspended by temperatures below 10° C., and as long as they are reduced to torpor the beer remains untainted either by acidity or putrefaction. The beer of low fermentation is brewed in winter, and kept in cool cellars; the brewer being thus enabled to dispose of it at his leisure, instead of forcing its consumption to avoid the loss involved in its alteration if kept too long. Hops, it may be remarked, act to some extent as an antiseptic to beer. The essential oil of the hop is bactericidal: hence the strong impregnation with hop juice of all beer intended for exportation.

These low organisms, which one might be disposed to regard as the beginnings of life, were we not warned that the microscope, precious and perfect as it is, has no power to show us the real beginnings of life, are by no means purely useless or purely mischievous in the economy of nature. They are only noxious when out of their proper place. They exercise a useful and valuable function as the burners and consumers of dead matter, animal and vegetable, reducing such matter, with a rapidity otherwise unattainable, to innocent carbonic acid and water. Furthermore, they are not all alike, and it is only restricted classes of them that are really dangerous to man. One difference in their habits is worthy of special reference here. Air, or rather the oxygen of the air, which is absolutely necessary to the support of the bacteria of putrefaction, is absolutely deadly to the vibrios which provoke the butyric acid fermentation. This is most simply illustrated by the following beautiful observation of Pasteur. You know the way of looking at these small organisms through the microscope. A drop of the liquid containing them is placed upon glass, and on the drop is placed a circle of exceedingly thin glass; for, to magnify them sufficiently, it is necessary that the microscope should come very close to the organisms. Round the edge of the circular plate of glass the liquid is in contact with the air, and incessantly absorbs it, including the oxy-

gen. Here, if the drop be charged with bacteria, we have a zone of very lively ones. But through this living zone, greedy of oxygen and appropriating it, the vivifying gas cannot penetrate to the centre of the film. In the middle, therefore, the bacteria die, while their peripheral colleagues continue active. If a bubble of air chance to be enclosed in the film, round it the bacteria will pirouette and wabble until its oxygen has been absorbed, after which all their motions cease. Precisely the reverse of all this occurs with the vibrios of butyric acid. In their case it is the peripheral organisms that are first killed, the central ones remaining vigorous while ringed by a zone of dead. Pasteur, moreover, filled two vessels with a liquid containing these vibrios; through one vessel he led air, and killed its vibrios in half-an-hour; through the other he led carbonic acid, and after three hours found the vibrios fully active. It was while observing these differences of deportment fifteen years ago that the thought of life without air, and its bearing upon the theory of fermentation, flashed upon the mind of this admirable investigator.

And here I am tempted to inquire how it is that during the last five or six years so many of the cultivated English and American public, including members of the medical profession and contributors to some of our most intellectual journals, could be so turned aside as they have been from the pure well-spring of scientific truth to be found in the writings of Pasteur? The reason I take to be, that while against unsound logic a healthy mind can always defend itself, against unsound experiment, without discipline it is defenceless. To judge of the soundness of scientific data, and to reason from data assumed to be sound, are two totally different things. The one deals with the raw material of fact, the other with the logical textures woven from that material. Now the logical loom may go accurately through all its motions, while the woven fibres may be all rotten. It is this inability, through lack of education in experiment, to judge of the soundness of experimental work, which lies at the root of the defection from Pasteur.

I will cite an example of this mistake of judgment. Between the large-type articles and the reviews of the

Saturday Review essays on various subjects are interpolated. In the calm of holiday evenings, while reading these brief essays, I have been many a time impressed, not only with their sparkling cleverness, but with their deep-searching wisdom and their wealth of spiritual experience. In this central region of the *Review* the question of spontaneous generation has been taken up and discussed. The writer is not a whit behind his colleagues in literary brilliancy and logical force. But having no touchstone in his own experience to enable him to distinguish a good experiment from a bad one, he has, on a point of the gravest practical import, committed the influence of the powerful journal in which he writes to the support of error. It is only, I would repeat, by practice among facts that the intellect is prepared to judge of facts, and no mere logical acuteness or literary skill can atone for the want of this necessary education.

We now approach an aspect of this question which concerns us still more closely, and which will be best illustrated by an actual fact. A few years ago I was bathing in an Alpine stream, and returning to my clothes from the cascade which had been my shower-bath, I slipped upon a block of granite, the sharp crystals of which stamped themselves into my naked shin. The wound was an awkward one, but being in vigorous health at the time, I hoped for a speedy recovery. Dipping a clean pocket handkerchief into the stream, I wrapped it round the wound, limped home, and remained for four or five days quietly in bed. There was no pain, and at the end of this time I thought myself quite fit to quit my room. The wound, when uncovered, was found perfectly clean, uninflamed, and entirely free from matter. Placing over it a bit of goldbeater's skin, I walked about all day. Towards evening itching and heat were felt; a large accumulation of matter followed, and I was forced to go to bed again. The water-bandage was restored, but it was powerless to check the action now set up; arnica was applied, but it made matters worse. The inflammation increased alarmingly, until finally I was ignobly carried on men's shoulders down the mountain and transported to Geneva, where, thanks to the kindness of friends, I was

immediately placed in the best medical hands. Or morning after my arrival in Geneva, Dr Gautier disco an abscess in my instep, at a distance of five inches the wound. The two were connected by a channel, or s as it is technically called, through which he was ab empty the abscess, without the application of the lance.

By what agency was that channel formed—what w that thus tore asunder the sound tissue of my instep, kept me for six weeks a prisoner in bed? In the very where the water-dressing had been removed from my w and the goldbeater's skin applied to it, I opened this y number of tubes, containing perfectly clear and swee fusions of fish, flesh, and vegetable. These hermeti sealed infusions had been exposed for weeks, both to sun of the Alps and to the warmth of a kitchen, wit showing the slightest turbidity or sign of life. But days after they were opened the greater number of t swarmed with the bacteria of putrefaction, the germs of w had been contracted from the dust-laden air of the ro And had the matter from my abscess been examined, memory of its appearance leads me to infer that it w have been found equally swarming with these bacteri that it was their germs which got into my incautiously-ope wound, and that they were the subtle workers that rowed down my shin, dug the abscess in my instep, produced effects which might well have proved fatal to n

We here come face to face with the labours of a man has established for himself an imperishable reputation relation to this subject, who combines the penetration of true theorist with the skill and conscientiousness of the t experimenter, and whose practice is one continued dem stration of the theory that the putrefaction of wounds is be averted by the destruction of the germs of bacteria. only from his own reports of his cases, but from the repo of eminent men who have visited his hospital, and from opinions expressed to me by Continental surgeons, do gather that one of the greatest steps ever made in the art surgery was the introduction of the antiseptic system treatment, practised, first in Glasgow, and now in Ed burgh, by Professor Lister.

The interest of this subject does not slacken as we proceed. We began with the cherry-cask and beer-vat; we end with the body of man. There are persons born with the power of interpreting natural facts, as there are others smitten with everlasting incompetence in regard to such interpretation. To the former class in an eminent degree belonged the celebrated philosopher Robert Boyle, whose words in relation to this subject have in them the forecast of prophecy. "And let me add," writes Boyle in his "Essay on the Pathological Part of Physik," "that he that thoroughly understands the nature of ferments and fermentations shall probably be much better able than he that ignores them, to give a fair account of divers phenomena of several diseases (as well fevers as others) which will perhaps be never properly understood without an insight into the doctrine of fermentations."

Two hundred years have passed since these pregnant words were written, and it is only in this our day that men are beginning to fully realise their truth. In the domain of surgery the justice of Boyle's surmise has been most strictly demonstrated. Demonstration is indeed the only word which fitly characterises the evidence brought forward by Professor Lister. You will grasp in a moment his leading idea. Take the extracted juice of beef or mutton, so prepared as to be perfectly transparent, and entirely free from the living germs of bacteria. Into the clear liquid let fall the tiniest drop of an infusion charged with the bacteria of putrefaction. Twenty-four hours subsequently the clear extract will be found muddy throughout, the turbidity being due to swarms of bacteria generated by the drop with which the infusion was inoculated. At the same time the infusion will have passed from a state of sweetness to a state of putridity. Let a drop similar to that which has produced this effect fall into an open wound: the juices of the living body nourish the bacteria as the beef or mutton juice nourished them, and you have putrefaction produced within the system. The air, as I have said, is laden with floating matter which, when it falls upon the wound, acts substantially like the drop. Professor Lister's aim is to destroy the life of that floating matter—to kill such germs as it may

contain. Had he, for example, dressed my wound, instead of opening it incautiously in the midst of air laden with the germs of bacteria, and instead of applying to it goldbeater's skin, which probably carried these germs upon its surface, he would have showered upon the wound, during the time of dressing, the spray of some liquid capable of killing the germs. The liquid usually employed for this purpose is dilute carbolic acid, which, in his skilled hands, has become a specific against putrefaction and all its deadly consequences.

We now pass the bounds of surgery proper, and enter the domain of epidemic disease, including those fevers so sagaciously referred to by Boyle. The most striking analogy between a *contagium* and a ferment is to be found in the power of indefinite self-multiplication possessed and exercised by both. You know the exquisitely truthful figures regarding leaven employed in the New Testament. A particle hid in three measures of meal leavens it all. A little leaven leaveneth the whole lump. In a similar manner a particle of *contagium* spreads through the human body and may be so multiplied as to strike down whole populations. Consider the effect produced upon the system by a microscopic quantity of the virus of smallpox. That virus is to all intents and purposes a seed. It is sown as yeast is sown, it grows and multiplies as yeast grows and multiplies, and it always reproduces itself. To Pasteur we are indebted for a series of masterly researches, wherein he exposes the looseness and general baselessness of prevalent notions regarding the transmutation of one ferment into another. He guards himself against saying it is impossible. The true investigator is sparing in the use of this word, though the use of it is unsparingly ascribed to him; but, as a matter of fact, Pasteur has never been able to effect the alleged transmutation, while he has been always able to point out the open doorways through which the affirmers of such transmutations had allowed error to march in upon them.*

* Those who wish for an illustration of the care necessary in these researches, and of the carelessness with which they have in some cases been conducted, will do well to consult the Rev. W. H. Dallinger's excellent "Notes on Heterogenesis" in the October number of the *Popular Science Review*.

The great source of error here has been already alluded to in this discourse. The observers worked in an atmosphere charged with the germs of different organisms; the mere accident of first possession rendering now one organism, now another, triumphant. In different stages, moreover, of its fermentative or putrefactive changes, the same infusion may so alter as to be successively taken possession of by different organisms. Such cases have been adduced to show that the earlier organisms must have been transformed into the later ones, whereas they are simply cases in which different germs, because of changes in the infusion, render themselves valid at different times.

By teaching us how to cultivate each ferment in its purity,—in other words, by teaching us how to rear the individual organism apart from all others—Pasteur has enabled us to avoid all these errors. And where this isolation of a particular organism has been duly effected it grows and multiplies indefinitely, but no change of it into another organism is ever observed. In Pasteur's researches the Bacterium remained a Bacterium, the Vibrio a Vibrio, the Penicillium a Penicillium, and the Torula a Torula. Sow any of these in a state of purity in an appropriate liquid; you get it, and it alone, in the subsequent crop. In like manner, sow smallpox in the human body, your crop is smallpox. Sow there scarlatina, and your crop is scarlatina. Sow typhoid virus, your crop is typhoid—cholera, your crop is cholera. The disease bears as constant a relation to its contagium as the microscopic organisms just enumerated do to their germs, or indeed as a thistle does to its seed. No wonder, then, with analogies so obvious and so striking, that the conviction is spreading and growing daily in strength that reproductive parasitic life is at the root of epidemic disease—that living ferments finding lodgment in the body increase there and multiply, directly ruining the tissue on which they subsist, or destroying life indirectly by the generation of poisonous compounds within the body. This conclusion, which comes to us with a presumption almost amounting to demonstration, is clinched by the fact that virulently infective diseases have been discovered with which living organisms are as closely and as indissolubly

ıssociated as the growth of Torula is with the fermentation ɔf beer.

And here, if you will permit me, I would utter a word of varning to well-meaning people. We have now reached a ›hase of this question when it is of the very last importance hat light should once for all be thrown upon the manner in vhich contagious and infectious diseases take root and spread. 'o this end the action of various ferments upon the organs nd tissues of the living body must be studied; the habitat f each special organism concerned in the production of each pecific disease must be determined, and the mode by which s germs are spread abroad as sources of further infection. t is only by such rigidly accurate inquiries that we can btain final and complete mastery over these destroyers. Ience, while abhorring cruelty of all kinds, while shrinking mpathetically from all animal suffering—suffering which y own pursuits never call upon me to inflict, an unbiassed ırvey of the field of research now opening out before the ıysiologist causes me to conclude, that no greater calamity uld befall the human race than the stoppage of experiental inquiry in this direction. A lady whose philanropy has rendered her illustrious said to me some time ago, at science was becoming immoral; that the researches of e past, unlike those of the present, were carried on witht cruelty. I replied to her that the science of Kepler and ewton, to which she referred, dealt with the laws and enomena of inorganic nature; but that one great advance de by modern science was in the direction of biology, or science of life; and that in this new direction scientific uiry, though at the outset pursued at the cost of some ıporary suffering, would in the end prove a thousand es more beneficent than it had ever hitherto been. I l this because I saw that the very researches which the y deprecated were leading us to such a knowledge of lemic diseases, as will enable us finally to sweep these ırges of the human race from the face of this fair earth.
'his is a point of such special importance that I should to bring it home to your intelligence by a single trustthy illustration. In 1850 two distinguished French

observers, MM. Davainne and Rayer, noticed in the blood of animals which had died of the virulent disease called *splenic fever*, small microscopic organisms resembling transparent rods, but neither of them at that time attached any significance to the observation. In 1861 Pasteur published a memoir on the fermentation of butyric acid, wherein he described the organism which provoked it; and after reading this memoir it occurred to Davainne that splenic fever might be a case of fermentation set up within the animal body, by the organisms which had been observed by him and Rayer. This idea has been placed beyond all doubt by subsequent research.

Some years in advance of the labours undertaken by Davainne, observations of the highest importance had been made on splenic fever by Pollender and Brauell. Two years ago, Dr Burdon Sanderson gave us a very clear account of what was known up to that time of this disorder. With regard to the permanence of the contagium, it had been proved to hang for years about localities where it had once prevailed; and this seemed to show that the rod-like organisms could not constitute the contagium, because their infective power was found to vanish in a few weeks. But other facts established an intimate connection between the organisms and the disease, so that a review of all the facts caused Dr Sanderson to conclude that the contagium existed in two distinct forms: the one "fugitive" and visible as transparent rods; the other permanent but "latent," and not yet brought within the grasp of the microscope.

At the time that Dr Sanderson was writing this report, a young German physician, named Koch, occupied with the duties of his profession in an obscure country district, was already at work, applying, during his spare time, various original and ingenious devices to the investigation of splenic fever. He studied the habits of the rod-like organisms, and found the aqueous humour of an ox's eye to be particularly suitable for their nutrition. With a drop of the aqueous humour he mixed the tiniest speck of a liquid containing the rods, placed the drop under his microscope, warmed it suitably, and observed the subsequent action. During the first two hours hardly any change was noticeable; but at the

end of this time the rods began to lengthen, and the action was so rapid that at the end of three or four hours they attained from ten to twenty times their original length. At the end of a few additional hours they had formed filaments in many cases a hundred times the length of the original rods. The same filament, in fact, was frequently observed to stretch through several fields of the microscope. Sometimes they lay in straight lines parallel to each other, in other cases they were bent, twisted, and coiled into the most graceful figures; while sometimes they formed knots of such bewildering complexity that it was impossible for the eye to trace the individual filaments through the confusion.

Had the observation ended here an interesting scientific fact would have been added to our previous store, but the addition would have been of little practical value. Koch, however, continued to watch the filaments, and after a time noticed little dots appearing within them. These dots became more and more distinct, until finally the whole length of the organism was studded with minute ovoid bodies, which lay within the outer integument like peas within their shell. By-and-by the integument fell to pieces, the place of the organism being taken by a long row of seeds or spores. These observations, which were confirmed in all respects by the celebrated naturalist, Cohn of Breslau, are of the highest importance. They clear up the existing perplexity regarding the latent and visible contagia of splenic fever; for in the most conclusive manner, Koch proved the spores, as distinguished from the rods, to constitute the contagium of the fever in its most deadly and persistent form.

How did he reach this important result? Mark the answer. There was but one way open to him to test the activity of the contagium, and that was the inoculation with it of living animals. He operated upon guinea-pigs and rabbits, but the vast majority of his experiments were made upon mice. Inoculating them with the fresh blood of an animal suffering from splenic fever, they invariably died of the same disease within twenty or thirty hours after inoculation. He then sought to determine how the contagium maintained its vitality. Drying the infectious blood containing the rod-like organisms, in which, however, the spores

were not developed, he found the contagium to be that whicl Dr Sanderson calls "fugitive." It maintained its power o infection for five weeks at the furthest. He then drie blood containing the fully-developed spores, and exposed th substance to a variety of conditions. He permitted th dried blood to assume the form of dust; wetted this dust allowed it to dry again, permitted it to remain for an in definite time in the midst of putrefying matter, and subjecte it to various other tests. After keeping the spore-charge blood which had been treated in this fashion for four years he inoculated a number of mice with it, and found its actio as fatal as that of blood fresh from the veins of an anima suffering from splenic fever. There was no single escap from death after inoculation by this deadly contagium. Ur counted millions of these spores are developed in the body c every animal which has died of splenic fever, and ever spore of these millions is competent to produce the diseas The name of this formidable parasite is *Bacillus anthracis.**

Now the very first step towards the extirpation of thes contagia is the knowledge of their nature; and the knov ledge brought to us by Dr Koch will render as certain tl stamping out of splenic fever as the stoppage of the plagu of pébrine by the researches of Pasteur. One small item statistics will show what this implies. In the single distri of Novgorod in Russia, between the years 1867 and 187 over fifty-six thousand cases of death by splenic fever, amor horses, cows, and sheep, were recorded. But its ravages d not confine themselves to the animal world, for during tl time, and in the district referred to, five hundred ar twenty-eight human beings perished in the agonies of tl same disease.

* To produce its characteristic effects the contagium of splenic fev must enter the blood. The virulently infective spleen of a diseas animal may be eaten with impunity by mice. On the other hand, t disease refuses to be communicated by inoculation to dogs, partridg or sparrows. In their blood *Bacillus anthracis* ceases to act as a f ment. Pasteur announced more than six years ago the propagation the vitrios of the silkworm disease called *flacherie*, both by scissi and by spores. He also made some remarkable experiments on t permanence of the contagium in the form of spores. See "Etudes s la Maladie des Vers à Soie," pp. 168 and 256.

A description of the fever will help you to come to a right decision on the point which I wish to submit to your consideration. "An animal," says Dr Burdon Sanderson, "which perhaps for the previous day has declined food and shown signs of general disturbance, begins to shudder and to have twitches of the muscles of the back, and soon after becomes weak and listless. In the meantime the respiration becomes frequent and often difficult, and the temperature rises to three or four degrees above the normal; but soon convulsions, affecting chiefly the muscles of the back and loins, usher in the final collapse, of which the progress is marked by complete loss of power of moving the trunk or extremities, diminution of temperature, mucous and sanguinolent alvine evacuations, and similar discharges from the mouth and nose." In a single district of Russia, as above remarked, fifty-six thousand horses, cows, and sheep, and five hundred and twenty-eight men and women, perished in this way during a period of two or three years. What the annual fatality is throughout Europe I have no means of knowing. Doubtless it must be very great. The question, then, which I wish to submit to your judgment is this: Is the knowledge which reveals to us the nature, and which assures the extirpation, of a disorder so virulent and so vile, worth the price paid for it? It is exceedingly important that assemblies like the present should see clearly the issues at stake in such questions as this, and that the properly-informed common sense of the community should temper, if not restrain, the rashness of those who, meaning to be tender, would virtually enact the most hideous cruelty by the imposition of short-sighted restrictions upon physiological investigation. It is a modern instance of zeal for God, but not according to knowledge, the excesses of which zeal an instructed public opinion must correct.

And now let us cast a backward glance on the field we have traversed, and try to extract from our labours such further profit as they can yield. For more than two thousand years the attraction of light bodies by amber was the sum of human knowledge regarding electricity, and for more than two thousand years fermentation was effected without

any knowledge of its cause. In science one discovery grow out of another, and cannot appear without its proper ant cedent. Thus, before fermentation could be understood, th microscope had to be invented and brought to a considerabl degree of perfection. Note the growth of knowledg Leeuwenhoek, in 1680, found yeast to be a mass of floatin globules, but he had no notion that the globules were aliv This was proved in 1835 by Cagniard de la Tour and Schwan Then came the question as to the origin of such microscop organisms, and in this connection the memoir of Pasteu published in the "Annales de Chimie" for 1862, is epoc making, proving as it did to all competent minds spontaneo generation to be thus far a chimera. On that investigati all Pasteur's subsequent labours were based. Ravages h over and over again occurred among French wines. The was no guarantee that they would not become acid or bitt particularly when exported. The commerce in wines w thus restricted, and disastrous losses were often inflicted the wine-grower. Every one of these diseases was traced the life of an organism. Pasteur ascertained the temperatu which killed these ferments of disease, proving it to be low as to be perfectly harmless to the wine. By the sim expedient of heating the wine to a temperature of fifty degr centigrade, he rendered it inalterable, and thus saved country the loss of millions. He then went on to vinegar *vin aigre*, acid wine—which he proved to be produced b fermentation set up by a little fungus called *Mycoderma ac* *Torula*, in fact, converts the grape juice into alcohol, a *Mycoderma aceti* converts the alcohol into vinegar. H also frequent failures occurred and severe losses were s tained. Through the operation of unknown causes, vinegar often became unfit for use, sometimes indeed fall into utter putridity. It had been long known that m exposure to the air was sufficient to destroy it. Past studied all these changes, traced them to their living cau and showed that the permanent health of the vinegar ensured by the destruction of this life. He passed from diseases of vinegar to the study of a malady which a do years ago had all but ruined the silk husbandry of Fra This plague, which received the name of *pébrine*, was

product of a parasite which first took possession of the intestinal canal of the silkworm, spread throughout its body, and filled the sack which ought to contain the viscid matter of the silk. Thus smitten, the worm would go automatically through the process of spinning when it had nothing to spin. Pasteur followed this parasitic destroyer from year to year, and, led by his singular power of combining facts with the logic of facts, discovered eventually the precise phase in the development of the insect when the disease which assailed it could with certainty be stamped out. Pasteur's devotion to this inquiry cost him dear. He restored to France her silk husbandry, rescued thousands of her population from ruin, set the looms of Italy also to work, but emerged from his labours with one of his sides permanently paralysed. His last investigation is embodied in a work entitled "Studies on Beer," in which he describes a method of rendering beer permanently unchangeable. That method is not so simple as those found effectual with wine and vinegar, but the principles which it involves are sure to receive extensive application at some future day. Taking into account all these labours of Pasteur, it is no exaggeration to state that the money value of his work would go far to cover the indemnity which France had to pay to Germany.

There are other reflections connected with this subject which, even were I to pass them over without remark, would sooner or later occur to every thoughtful mind in this assembly. I have spoken of the floating dust of the air, of the means of rendering it visible, and of the perfect immunity from putrefaction which accompanies the contact of germless matter and moteless air. Consider the woes which these wafted particles, during historic and pre-historic ages, have inflicted on mankind; consider the loss of life in hospitals from putrefying wounds; consider the loss of life in places where there are plenty of wounds but no hospitals, and in the ages before hospitals were anywhere founded; consider the slaughter which has hitherto followed that of the battle-field, when those bacterial destroyers are let loose, often producing a mortality far greater than that of the battle itself; add to this the other conception that in times of epidemic disease the self-same floating matter has fre-

quently, if not always, mingled with it the special gern which produce the epidemic, being thus enabled to so pestilence and death over nations and continents—consid all this, and you will come with me to the conclusion th all the havoc of war, ten times multiplied, would be eva escent if compared with the ravages due to atmospher dust.

This preventible destruction is going on to-day, and it h been permitted to go on for ages, without a whisper of i formation regarding its cause being vouchsafed to the su fering sentient world. We have been scourged by invisib thongs, attacked from impenetrable ambuscades, and it only to-day that the light of science is being let in upon t murderous dominion of our foes. Men of Glasgow, fac like these excite in me the thought that the rule and g vernance of the universe are different from what we in o youth supposed them to be—that the inscrutable Power, once terrible and beneficent, in whom we live and move a have our being and our end, is to be propitiated by mea different from those usually resorted to. The first requis towards such propitiation is *knowledge;* the second is *actic* shaped and illuminated by that knowledge. Of knowled we already see the dawn, which will open out by-and-by perfect day, while the action which is to follow has its u failing source and stimulus in the moral and emotio nature of man—in his desire for personal well-being, in l sense of duty, in his compassionate sympathy with the s ferings of his fellow-men. "How often," says Dr Willi Budd in his celebrated work on Typhoid Fever,—"h often have I seen in past days, in the single narrow cha ber of the day-labourer's cottage, the father in the coffin, t mother in the sick-bed in muttering delirium, and nothi to relieve the desolation of the children but the devotion some poor neighbour, who in too many cases paid the pena of her kindness in becoming herself the victim of the sa disorder." From the vantage-ground already won I lo forward with confident hope to the triumph of medical over scenes of misery like that here described. The ca of the calamity being once clearly revealed, not only to t physician, but to the public, whose intelligent co-operat

is absolutely essential to success, the final victory of humanity is only a question of time. We have already a foretaste of that victory in the triumphs of surgery as practised at your doors.

And here, ladies and gentlemen, my words ought to cease. I have endeavoured to unfold in your presence discoveries and doctrines which have a special bearing on the life of great cities such as this, and which have a still wider bearing on the welfare of the human race. I regard it as a high privilege to have had the opportunity of meeting you here. I thank you for the courtesy you have extended to me. I wish prosperity to your association, long life to its president, and to each and all of you I bid a friendly farewell.

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THE ANTIQUITY OF THE CAVE MEN.

If I may flatter myself that there are now present in this hall at least a tolerable number of persons who have a recollection of the lecture which I delivered here in December 1875, I may safely also assume that there are likewise present several questions suggested by that lecture; and that amongst these questions this is probably one of the most prominent—When did these Devonshire Cave-Men live? In other words, what is the antiquity of man in Britain? This is the subject on which I am to have the pleasure of addressing you this evening. I may tell you at once, however, that I can make no attempt, for I should be an impostor if I did so, to say what is the numerical value of the time separating us from the old cave men of whom I spoke in my last lecture. We cannot at present reduce geological to astronomical time, and we never may be able to do so. We cannot say what is the value in years of a deposit a foot thick, or any other thickness; but, if we look at the question properly, we shall find a feeling of time taking possession of the mind, and we may go away with a correct impression, though a vague one, that our fathers vastly undervalued the antiquity of man. Professor Prestwich expressed in very excellent terms the idea which possesses my mind, in a paper read to the Royal Society on this very question on 19th June 1862. His words were: "Just as, though ignorant of the precise height and size of a mountain range seen in the distance, we need not wait for trigonometrical measurement to feel satisfied in our minds of the magnitude of the distant peaks; so with this geological epoch, we see and know enough of it to feel how distant it is from our time, and yet we are not in a position at present to solve

with accuracy the curious and interesting problem of its precise age." Such was the language of Professor Prestwich, and, I believe, it is the language we must still employ; but since these words were written, we have discovered traces of earlier men still than were at that time contemplated.

There are at least five lines of inquiry, five classes of evidence, on the question before us. These are (1) the *geological*—the deposits we have to deal with; (2) the *biological*—the animals with whose remains we have met; (3) the *archæological*—the human industrial remains we find in the deposits; (4) the *geographical* evidence, which is twofold, viz., (*a*) changes in the geography of the district surrounding, say, Kent's Hole, for my lecture will be mainly on that famous cavern, and (*b*) changes in the relation of this country to the Continent; and (5) the *climatological* evidence, that is to say, the indications of climate given by the bones of the animals we have exhumed. Each one of these five lines of evidence would be amply sufficient for a lecture; but, instead of giving a lecture on each of these points, I shall, aided by the following table, go through as many of them as I can on this occasion. I cannot, however, hope to go beyond the first three, and must leave the geographical and climatological evidence untouched, and open, perhaps, for some future opportunity.

You will observe that the table consists of two divisions, separated with double vertical lines, and each containing three columns. The first, or left hand division, relates exclusively to Kent's Cavern, as is indicated by the words heading it. The second, or right hand division, headed "Periods," takes us farther a-field, and is of a more general character; and the task before me is mainly to explain the two sets of columns, and show their chronological relations. The left hand column of the first division gives the deposits of the cavern in descending order, viz., *Black Mould, Granular Stalagmite, Black Band, Cave Earth, Crystalline Stalagmite,* and *Breccia.* These are the six deposits met with in Kent's Cavern, to say nothing about huge masses of limestone, some of them weighing 100 tons and upwards, which have fallen from the roof from time to time.

I fancy some one is asking if it be not possible that in at

Kent's Cavern.			Periods.		
Deposits.	Bones.	Implements.	Archæological.	Danish Bog.	Biological.
		Iron,	Iron.	Beech.	
				Pedunculated Oak.	
Black Mould.	Ovine.	Bronze,	Bronze.		Recent.
		and (?)		Sessile Oak.	
		Neolithic.	Neolithic.	Scotch Fir.	
Granular Stalagmite.					
Black Band.	Hyænine.	Palæolithic Flakes.			
Cave Earth.			Palæolithic.		Pleistocene.
Crystalline Stalagmite.					
	Ursine.	Palæolithic Nodules.			
Breccia.					

least some of these deposits there may be what, for lack of a better expression, may be called "potted anachronisms,"—things belonging to different periods, yet lying together. The geologist knows perfectly that he has to guard against cases of that kind. Let me give an illustration: There is in North Devon a little Dartmoor river called the Lew. Near the town of Hatherleigh it meanders through a considerable alluvial plain, which it formed in the past. It happens that on the plain there are a great many oak and other trees growing, some of them 3 feet in diameter. It is clear that although the river has from time to time wandered everywhere over that plain, it has left untouched the spots which are occupied by the oaks ever since the germination of the acorns from which they sprang. One of these oaks, fully 3 feet in diameter, was situated close to the river's edge, and it happened that the river had so encroached as to undermine that tree. Then came an envious storm and threw the tree obliquely across the stream. The river, being thus impeded, scooped out a great bight in the alluvial soil, and in doing so disclosed the trunk of an old oak, black as ebony, which had been prostrated there countless ages ago. Subsequently the river fell to its ordinary dimensions, and the disclosed old oak tree became in its turn an obstacle. The stream contented itself with flowing between the newly fallen oak and the old one, and began to silt up the latter a second time, perhaps a third time; and, whilst thus engaged, brought down from the higher country sundry "odds and ends"—a bottle, which my friend and I thought was a port wine bottle, part of an old tin kettle, and a variety of other articles, which were all deposited by the side of the old oak. I said to my friend, "Some future geologist will perhaps come here and find lying together an oak tree, a port wine bottle, and a tin kettle. He may think they all belong to the same age, and perhaps say to his friends, 'How very strange that men, who were so highly civilised as to know and appreciate port wine, should have neglected so valuable a thing as a large oak tree!' We, however, know better; the objects do not all belong to the same period." Did nothing of that kind occur in Kent's Hole? With this question before us, let us turn to the cavern deposits.

These, if the cave earth and black band be taken as one, are five in number, of which three—the black mould, cave earth, and breccia—are of mechanical derivation, whilst the remaining two, the granular and crystalline stalagmites, are of chemical origin, having been formed by the limestone overhead being dissolved by water containing carbonic acid, and *precipitated* molecule by molecule on the successive floors of the cavern.

Now, it will be observed that a thick chemical sheet lies between each consecutive pair of mechanical beds: thus, the granular stalagmite separates the black mould above it from the cave earth below it, and, in like manner, the crystalline stalagmite divides the cave earth from the breccia. In other words, the breccia had been deposited and hermetically sealed and secured before the introduction of the cave earth commenced; and this, again, was completely covered and protected before the first instalment of the black mould was laid down.

It will be seen from the nature of the case that the anachronisms, if such there be, must be confined to the *mechanical* beds, for the objects incorporated within the stalagmites were at once fixed in their places by the cementing action of the calcareous matter which entombed them, and which, unlike incoherent matter mechanically brought together, was incapable of disturbance or rearrangement.

Now, whilst it cannot be denied that specimens not strictly contemporary may be lying "cheek by jowl," as we say, in a mechanical bed, it must also be admitted that any anachronisms that may have occurred must be confined to the period which the bed as a whole represents. In short, everything in the cave earth, for example, must be more ancient than anything in the immediately overlying stalagmite, and more modern than anything in the crystalline sheet directly below; and so on in other cases.

It must also be obvious that any anachronisms which may exist can be of little moment in the question before us, unless it be admitted that the bed in which they are potted represents a very protracted period; but such an admission would be equivalent to acknowledging the great human antiquity for which I contend.

Let us now proceed with the beds in the order in which they occur. The black mould is found only in those parts of the cavern into which the entrances immediately open, and such as are prolongations of them. It varies in thickness from three inches to about a foot, and is made up mainly of vegetable matter, leaves blown into the cavern during autumn, articles left by visitors, things deposited by human dwellers in the cavern, or brought in by the smaller animals which lived there. Persons who now go into the cavern occasionally drop sixpences and other small objects, so that you perceive we were not unlikely to meet with anachronisms in that deposit; but I contend again that all the things found there belong to different parts of that one period represented by the black mould itself. They mainly and essentially, however, belong to the Romano-British and pre-Roman portions of British history. This uppermost bed, then, takes us back at least 2000 years as a minimum, and it is difficult to prove that it does not take us back much further. This is our first chronological stepping-stone.

We come next to the granular stalagmite, the second deposit. Kent's Cavern is an isolated hill. The rain that has done the work of forming the stalagmite is simply that which falls on the hill itself. There is no drainage from the surrounding country into the cavern. You know that a given quantity of water will take up a definite limited quantity of carbonic acid, the exact quantity depending partly on the temperature of the water and partly on the pressure to which the water is subjected. The higher the temperature of the water the less carbonic acid will it take up; and the greater the pressure the more it will take up. Speaking roughly, at ordinary pressures and temperatures I believe the water will take up, pretty much, volume for volume. It does not however follow that, because it can take up this amount, it can get it to take. The only carbonic acid which it gets is that derived from the atmosphere and from the decomposing vegetable matter on the surface. Armed with this, the water, percolating through the roof, dissolves the limestone, or, to speak scientifically, the carbonate of lime of which the roof consists, and the greater portion drops finally on the floor, where it takes one or other of three different

principal forms, "paps," bosses, and sheets, as they
called in Devonshire.

The pap, so named from its resemblance to a cow's pa
teat, rises from the floor in the form of a more or less
cylinder, of variable height and of comparatively sle
girth. So far as I know, most paps have a tube or
running down the centre from top to bottom. They
to be formed in this way: the water is dropped so
truly as to enter that central tube and occasionall
fill it, when a slight accretion takes place, and so the
gradually increases in height. The formation of the pa
the result of a very slow drip of water. Bosses are rou
in the form of paraboloids, and have their origin in a
copious drip of water; and a still more plentiful
forms a sheet. All the three formations, however, are e
sively slow. Now, if we ascertain the rate at which a g
pap is formed, that may serve very well to enable us to
culate the amount of time represented by *that* pap, b
will not necessarily apply to another pap, and certainly
not be used as a chronometer in the case of a paraboloi
a sheet. Going a little further, suppose the drip from
roof had been more copious still, a flowing stream w
have resulted, and probably no stalagmite whatsoever w
have been precipitated, as the calcareous matter would
been carried out of the cave.

It is held by many that stalagmite is formed in cave
cause the water evaporates, and leaves a calcareous resid
What if there be no evaporation? I have wet and dry
thermometers in Kent's Cavern, and so far as I have
discovered, there is, except near the entrances, no
poration at all. How the stalagmite is formed is
perhaps a moot point. My hypothesis—for it dese
no higher name—is this: The temperature of the cav
almost unvarying; there is no summer or winter in
no day and no night, so far as heat is concerned. All
year round the temperature stands at about $51\frac{1}{2}°$.
suppose that in the winter time the temperature of
exterior be some 35° or 40°. The water falling on the
licks up the carbonic acid from the surface, and dissolve
much limestone as it can. It reaches the interior satur

with carbonate of lime, and finding the temperature there $51\frac{1}{2}°$, rises to that temperature. That increased temperature throws off a portion of the carbonic acid, and carbonate of lime is necessarily precipitated. If this hypothesis be correct, the deposit is formed in the winter season only, and never in the summer, except near the entrances of the cavern. This speculation I throw out for what it is worth; but if I am right, the rate at which the stalagmite is formed is excessively slow indeed.

A good deal has been said about the rate at which the masses of stalagmite in Ingleborough Cave, in Yorkshire, have been formed, and a friend of mine has made some interesting observations on the mass called the Jockey Cap. I saw that mass last September, and am perfectly satisfied that the measurements of it just alluded to are utterly valueless for a measurement of the time represented by anything else than the Jockey Cap itself. That chronometer does not apply to anything else. It cannot apply to anything in Kent's Cavern. The rate at which a mass is formed in one cave is not necessarily or probably the rate at which a mass has been formed in another cave, or even in another branch of the same cave.

In Kent's Cavern there is a series of inscriptions on a boss, one of which goes back to 1604. I do not like people to cut their names on trees, etc.; I should not like to see John Smith's name on the top of an Egyptian pyramid; but I am thankful that people had this habit of old, and that some of them cut their names, initials, and dates, 270 years ago in Kent's Cavern. That inscription is not obliterated. Stalagmite has accreted on it, but the amount formed upon it during the last 270 years is certainly not more than a twentieth of an inch in thickness. Measured from the centre of the boss on which it occurs, and at right angles to the successive almost filmy sheets, there are 5 feet to account for and 270 years for the twentieth of an inch. The time now confronting us seems almost overwhelming. It will be remembered that the black mould took us back at least 2000 years before we reached this stalagmite deposit; and we cannot but feel that we may rest the whole question of the antiquity of man being greater than our fathers supposed on these, our first two

stepping-stones alone. But some of my friends tell me that possibly stalagmite was formed more rapidly in ancient times. Admit the possibility, but let us see where it will lead us. Was there more rain in those olden days? If so, as no more rain can come down than has gone up in the form of vapour, there must have been more evaporation and a higher temperature; but the temperature was lower during the Kent's Hole period, not higher. Was there more carbonic acid in the atmosphere? Geology affords no evidence of anything of the kind. Did vegetation grow more rapidly, and, by its decomposition, produce more carbonic acid? This, too, would imply more heat, which we have seen is an untenable position. Was the configuration of the surface of the surrounding district such that the cavern received the drainage of a large area, and not simply that of the hill in which it is situated? That is simply another, and probably not a shorter road into antiquity. Supposing that any or all of these things are assumed, they inevitably absorb a large amount of time and that is the very thing for which we are contending. I do not believe it is possible to take any view of the formation of the stalagmite that will require a less amount of time than that which I have just endeavoured to put before you, independently of these hypotheses of which I have just spoken. I ought to state, as one general fact, that wherever in the cavern the drip from the roof is rapid, there the stalagmite is thick, implying that it was rapid formerly as now; wherever the stalagmite is thin there is now but little drip; and wherever, and there are few such places, there is no stalagmite at all there is no drip at all. The *lines* of drainage, then, whatever may be the truth about the *rate* of drainage, have remained the same throughout all the time represented by the stalagmitic floor and bosses, and paps.

Before quitting this part of the subject it seems desirable to direct attention to the necessity for great caution in the use of data when attempting to calculate the time represented by stalagmites. By way of illustration, and in order to secure simplicity, let it be supposed that the *volumes* of stalagmitic matter, precipitated in given large equal periods have been uniformly the same, so far as any particular ma

to be dealt with is concerned. Let fig. 1 be a vertical section through a horizontal *sheet* of stalagmite of uniform thickness built up on a well-defined area by the successive formation of parallel and horizontal laminæ, represented by the parallel lines in the figure; and let the line *ab*, drawn at right angles to the laminæ, and therefore representing the thickness of the sheet, be 5 feet. Further, let it be supposed that it has been clearly ascertained, through inscriptions or other trustworthy evidence, that during the last 270 years the film precipitated amounted to ·05 (one-twentieth) of an inch in thickness, to use the figures already mentioned. In such a case no possible error could arise in using that thickness as a chronometer, and the conclusion that the whole sheet represented 324,000 years (= 5 feet ÷ ·05 inch × 270 years) would be perfectly trustworthy.

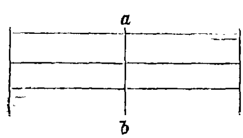


Fig. 1.

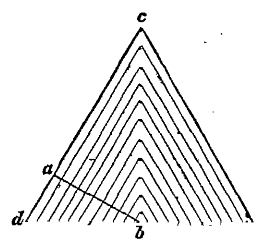


Fig. 2.

Next, let fig. 2 represent a vertical section through a *boss* in the form of a right cone, built up by a succession of symmetrical conical envelopes, represented by the parallel lines on each side of the axis, *b c*, of the figure, having its slant sides, *c d*, *c e*, and the diameter, *d e*, of its circular base, all equal to one another, and of such dimensions that the height, *b c*, of the cone is 10 feet. The line *a b*, drawn at right angles to the laminæ from the centre of the base, would be 5 feet; that is, the thickness to be accounted for would be the same as in fig. 1; but if, in this case also, the envelope formed during the last 270 years were known with certainty to be ·05 inch, the 5 feet would be found to be by no means equivalent to 324,000 years, as in the former case; for the

conical envelope, ·05 inch thick, and entirely and uniformly covering our cone, would contain about ·87267 cubic foot of stalagmite; and this, according to the hypothesis, would be the *volume* precipitated in every 270 years. This amount, however, would have made a thicker and thicker envelope when the cone was smaller and smaller; in other words, a film ·05 inch thick, instead of representing 270 years throughout the entire history of the cone, would be the index of a less and less period as it was followed further and further into antiquity, so that the chronological value of the entire cone must be calculated by another method—by *volume*, not *thickness*. The cone here supposed would contain about 349·0666 cubic feet, which, at the rate of ·87267 cubic foot in every 270 years, would give 108,000 years (= 349·0666 cubic feet ÷ ·87267 cubic foot × 270 years), instead of 324,000 years, as in fig. 1. It must be unnecessary to say that the foregoing figures must be regarded, not as actual, but as illustrative merely.

I pass now to the third of our stepping-stones—the cave earth and black band, which may be taken as one. The black band is essentially charcoal; in it we found flint tools and chips, bone tools, bones that had been roasted, and a variety of other evidences of human residence. There is an opinion prevalent in some minds that the cave earth was furnished in this way: If I were to throw this piece of chalk (that is, carbonate of lime) into dilute muriatic acid, the greater portion of it would be dissolved; but there would be an insoluble clayey residuum. Carbonate of lime is not, as we find it, generally pure. Now, the limestone forming the roof and wall of the cavern, having the carbonate of lime dissolved out, would leave some earthy residuum; and I am occasionally asked to believe that this is the only source from which the cave earth has been derived! I do not say there has not been a trifle, but it is the smallest trifle, of cave earth formed in that way; for a stalagmitic floor 5 feet thick represents the solution of a large amount of limestone overhead, and any earthy residuum ought to have been incorporated in that stalagmite, which, however, has usually no trace of it, but is entirely free from anything of the kind. Hence we feel perfectly satisfied that this earthy residuum

must have contributed very little to the formation of the cave earth. The answer to the question, Whence did it come? is, It came from the exterior. Moreover, it did not come through the roof; there are no visible cracks or fissures to allow of its passage; but it was washed in through those openings by which we now enter the cavern, day after day, and it was introduced in very small instalments. The proof of the last assertion is this: If you were to examine some of the objects, whether bones or stones, found in the cave earth, you would see that there are films of stalagmite investing them. What is the explanation of this prevalent fact? I understand it to signify that the cave earth, at all its successive levels, was for a time the floor of the cavern. On that floor there lay, let us say, this bone, along with a number of others, and stones and flint implements scattered here and there. Some of them, but not all, were exposed to the drip from the roof, and that drip left on it a little carbonate of lime, and invested it with the film. Then there was the introduction of a very small amount of cave earth which stopped the process, and the film grew no thicker. Half an inch or so above it you will find the same thing repeated, and so on from bottom to top of the entire thickness of the cave earth, showing clearly that the deposit was introduced through the entrances, and in very small instalments. Some of the flint implements found in the cave have edges so keen you might almost use them as razors; and if the transporting water had rushed in impetuous volume into the cave, carrying mud and stones before it, is it conceivable that these fine edges would have been preserved? I know to my cost that the slightest inattention to the delicate handling of the implements will break the edges, and yet we find them as keen and unbroken as it is possible for them to be.

These facts appear to show clearly that the cave earth was introduced in minute quantities with long periods of time intervening; and, therefore, the cave earth, with its black band, represents a vast period of time. Remember, we had to go back at least 2000 years in order to reach the close of the formation of the granular stalagmite; we had also to pass through the vast period represented by that sheet, and now we have a large amount of

still earlier time representing the black band and cave earth.

Below the cave earth we have the crystalline stalagmite, which is thicker than the granular, usually in the proportion of about 12 to 5. The same argument applies to the one sheet as to the other, but with this difference, that if the time represented by the granular stalagmite be so great, the time represented by the crystalline stalagmite must, in all probability, have been proportionately greater. We therefore make another great demand for time, for this lower and older stalagmite.

We next come, in descending order, to the breccia, and I may tell you that, while the cave earth consisted of light red clay loam, with about 50 per cent. of limestone. The breccia was made up of a dark red sandy paste, with singularly few fragments of limestone in it; but instead thereof, angular and rounded pieces of dark red grit—material which the limestone could not have supplied, nor the cavern hill have furnished.

I must ask you to remember that all the way back through the black mould, granular stalagmite, black band, cave earth, crystalline stalagmite, and breccia, we have clear unmistakable evidence of the existence of man.

I come now to the *biological* evidence; that furnished by the remains of the animals met with in the cave. I am afraid this part of the subject is scarcely possible of being thrown into a popular form, as it requires a somewhat more intimate acquaintance with biology than perhaps one can assume to exist in a miscellaneous audience. What is the biology of the black mould? The animals, whose remains we meet with in that uppermost deposit, are the seal, water-rat, rabbit, hare, goat, sheep, red deer, short-fronted ox (represented, it is believed, by the Welsh runt of the present day), brown bear, fox, dog, pig, and man. There are plenty of human bones in this upper deposit. I would ask you to note that there is not a single extinct animal in that list. They are all animals which still occupy Western Europe, and, with very few exceptions, are all such as occupy Devonshire at the present day.

Passing to the animals found in the granular stalagmite,

black band, and cave earth, we have very different types: the cave lion, lynx (?), wild cat, cave hyena, wolf, fox, Arctic fox (?), glutton, badger, cave bear, grizzly bear, brown bear, mammoth, the woolly rhinoceros, horse, wild bull, bison, Irish deer, reindeer, hare, cave pika, water vole, field vole, bank vole, beaver, and *machairodus latidens.* These animals are all found in the granular stalagmite, black band, and cave earth; but they are more abundant in the cave earth than in the other two deposits. In connection with this list there are one or two facts worth noticing. First, it includes animals that still exist in Devonshire; secondly, animals that no longer exist in Britain, but exist elsewhere in Western Europe, such as the brown bear and others; and, thirdly, animals that no longer exist in the world, and have not done so within the times of history or even tradition. Legends go back a great way, and sometimes clever and learned men explain the allusions they contain; but there is no allusion to such animals in any legends or traditions which are extant, to say nothing of trustworthy history.

Going further back still, to the crystalline stalagmite and breccia: the remains found in the former are few in number, and in the latter abundant. The cave bear is found in great plenty, the cave lion very sparingly, the fox still more sparingly. These are the only animal remains found there.

You perceive that these facts justified me in drawing out the second column in the table—that under the word "Bones." The first or upper section in this column is entitled "Ovine," and is set apart for the remains of animals found in the black mould, below which no relics of sheep were found. The remains found in the granular stalagmite, black band, and cave earth, embrace [illegible]he hyena in prodigious quantity; and I have therefore c[illegible] the period represented by these three deposits the "H[illegible]ne period." The hyena, I may mention, was not found above the granular stalagmite, nor below the cave earth. I think it not impossible that, in order to make myself clear, I may repeat from time to time some of the facts which I stated in my former lecture. The hyena, as you know, has comparatively few teeth in its head, as compared with the horse, the rhinoceros, and many other

forms. If you take all the teeth found in the cave earth, black band, and stalagmite, those of the hyena, leaving out of sight the paucity of teeth in its jaws, amount to 30 or 40 per cent. of the whole, showing clearly enough that he was master of the situation, the presiding spirit, and that he was in the habit of dragging piecemeal the remains of other animals into the cave to satisfy his hunger. The remains in the crystalline stalagmite and the breccia are almost exclusively those of bears; hence I term the time they represent the "Ursine period." The bear, however, was not *confined* to the crystalline stalagmite and breccia, as it was found in the hyenine deposits also. This biological classification is intended to apply to Kent's Hole only.

In considering this part of the subject, one is prompted to put the question, What was it caused the extinction from the entire world of many of the animals that have been named? Will you say that man killed them off? It would perhaps be not surprising that man should have killed off, say, the large mammoth. He has, however, failed to exterminate the allied species, the African and Asiatic elephants. The mammoth was probably not so very large an animal as many people suppose; its tusks were of enormous size, and hence the popular belief about their owner. It was, however, a large target, and even a bad marksman might, it is true, kill it down; but we have to remember that it extended from Texas and the latitude of Rome, all the way to the Arctic Ocean, across the whole of North America, Europe, and Asia; and from all these enormous tracts of country, where formerly it existed in countless numbers, it has entirely disappeared. But, supposing that man could directly have caused the extinction of such an animal as that, what about the little pika, the tail-less hare? I should not be surprised if there is at least one gentleman in this hall whose premises are infested with rats. When that gentleman can succeed in ridding his premises of these vermin with a certainty that they will never return, I will then believe it to have been possible for man, by direct action, to exterminate from the entire world the cave pikas, smaller than rats; but until that is done, I shall be sceptical. It was probably not man's direct agency that caused the extermination; but

he may have assisted by his indirect agency. To illustrate what I mean, let me say that we have two kinds of rats in Britain, the black rat, our oldest friend, and the larger reddish-grey rat. The latter came here in a ship, it is supposed, without the intention of the men who brought it; and this large reddish-grey rat has killed down the black rat to such an extent that the latter is now very rarely seen. How did the grey rat accomplish this? By a stand-up fight? Nothing of the kind. I do not say they never do fight; but the stronger grey rat ate the rat-producing food, and starved out the other kind, which is now disappearing in consequence of getting little to eat. The fact is, there is a certain amount of rat food in the country, of which the grey rat is master, and the consequence is the other is becoming gradually exterminated. I may state, in passing, that an eminent Frenchman told me (I do not know that it is a fact) that a species of rat, previously unknown in France, entered that country, following the French army in the disastrous retreat from Russia. I daresay a struggle for existence took place between that rat and the rats which were in France previously, and in that indirect way man may have had something to do with rendering the previous rats very scarce.

As already stated, we find, commingled in the hyenine period, animals, some of which still exist in Britain, others living in western continental Europe, and some totally extinct. It is fair to conclude that the last did not become extinct through any convulsion extending throughout the whole world. Any such convulsion that shook certain species out of the globe, would in all probability make a clean sweep of the whole, whereas only some kinds disappeared, while others remained. I do not believe that there has been, from the first advent of life in this world, anything like a universal and synchronous depopulation of the globe. If, therefore, it was not by convulsion, if it was through the failure of a particular kind of food, however caused, which starved out certain forms, we cannot suppose that the extinct animals, and there are a considerable number of them, all disappeared at one and the same time, or in anything like a short period. If any of you have seen Exeter Cathedral, you will have observed that in its exquisitely beautiful west front there

are many niches filled with figures of kings, warriors, and saints, all made of the same kind of stone. Some of these figures decay more rapidly than others; some are ready to drop out; some indeed have done so, and their places have been supplied with new figures; and others remain tolerably fresh. That probably represents the outgoing of the different forms of life which have disappeared from our earth. We cannot suppose that any two of them disappeared at one and the same time, and, when we have so many extinctions to deal with, there can be no doubt whatever that the extinctions represent a great amount of time.

Passing to another point: if you turn to the second division of the table—that headed "Periods," you will find in the last column, under the word "Biological," the words "Recent" and "Pleistocene." By "recent" we mean that all the animals, from shell-fish to mammals, of the period which it embraces, were identical with species now existing. By going back far enough in the deposits forming the earth's crust we come to a time when, although the shell-fish were all *specifically* identical with such as now exist, a portion, and sometimes a considerable portion, of the mammals belonged to species now extinct. Understand that I am not speaking of *individual* life, but *specific* life. Let us suppose that we take, by way of illustration, a cuttle-fish, with which you are all well acquainted, a star-fish, and, shall we say, a fox? The star-fish has the lowliest organization of the three, the cuttle-fish occupies an intermediate position, and the fox is more highly organised than either of the other two. They are all living now; but let us trace these animals back until we come to a time when they were not, a time, so to speak, when they made their advent. Which will carry us farthest back? The least highly organised—the star-fish. The specific life of a lowly organised creature, having all its parts but little differentiated, is longer than the specific life of a creature more highly organised. Thus, when we go back we lose more and more of our mammals, but still have all the shell-fish precisely as they are at the present day. When we go back to a time when the molluscs, that is, the shell-fish, were all, but the mammals were not all, identical with those of the present day, we call that period the "pleistocene period," as shown

in the table. There are numerous other periods farther back in time; but, without troubling ourselves about them, every one, I am sure, will admit that even to go back as far as the pleistocene era, is to take a vast journey into antiquity; but even there we find distinct evidences of the Devonshire Cave men, as is seen by a glance at the third and sixth columns of the table.

We now come to the *archæological* evidence. You will observe that, under the word "Implements," in the third column of the table, a general idea is given of the implements found in the successive cave deposits. The following is a list of the human industrial remains we found in the ovine deposit:—Whetstones; angular and curvilinear plates of slate; pieces of smelted copper; bronze articles, including rings, a brooch, a spear-head, and a pin; flint "strike-lights;" spindle whorls made of various kinds of stone, and some of them ornamented; bone tools, including an awl, a chisel, combs, in size and shape somewhat like shoe-horns having the teeth at the broad end; amber beads; charred wood; and—here is a case of the "potted anachronisms" previously alluded to—a halfpenny of 1806 and a sixpence of 1846, so that we were not without our pecuniary rewards. These are the artificial articles found in the black mould.

In the hyenine deposits we discovered bone tools, including a needle or bodkin having a well-formed eye, a pin, three harpoons, a perforated tooth of a badger, probably to be strung with others as a necklace; whetstones; a "hammerstone;" unpolished flint *flake* tools, flint chips, flint "cores;" and "dead" shells of pectens, that is, single valves, the molluscs which formed and inhabited them having died, and other marine organisms having built their shells *within* these valves as they lay at the bottom of the sea. If you were to find that a man brought home a "dead" shell, say of an oyster, you would conclude that he did not bring it home for the sake of any food it contained, but to use it as a tool. You have, for instance, seen pecten shells used in the present day for scooping out sugar and other things. In one place in the cavern I found seven of those shells packed one in the other as neatly as any housemaid could pack her saucers,

and the whole seven were placed away in a recess in the wall which appeared to have served as a cupboard.

In the ursine deposits we found unpolished flint tools and nothing else. I said a while ago that in the hyenine deposits were found flint *flake*-tools, but the men of the ursine period did not strike off flakes from flint nodules, and form the flakes into tools; but they took the nodules of flint and chipped *them* into useful shapes. I will ask you to notice carefully the difference in the "finds." In the upper deposit we have metals and pottery and spindle whorls. In the hyenine deposits we have nothing of the kind; there does not seem, during that period, to have been any knowledge of metals, or of spinning, or of pottery, however rude. The people, however, had flint *flake*-tools and bone tools; they had needles or bodkins, awls, and pins, probably for fastening skins across the chest. Some poet, describing a savage, says—

> "The shaggy wolfish skin he wore,
> Pinned by a polished bone before."

(Whence that passage comes I have not been able to find; but it is quoted by MacEnery in his description of Kent's Cave). There was, however, neither any kind of bone tool nor of flint *flake*-tool in the ursine deposits; the only artificial objects were flint *nodule*-tools.*

Now, let us see if we cannot co-ordinate the cavern archæology with the prehistoric archæology of Western Europe generally. In the second division of the table there is a column headed "Archæological." I need not tell you dwellers on the Clyde-side that we are living in the Iron Age. By going far enough back, however, we reach a time when men had no knowledge of iron; and it would almost seem that the earliest iron that was used was meteoric iron. It seems a difficult thing to understand how a man without something like inspiration could ever have got the idea that out of ironstone he could get the beautiful metal which we call iron, and use so largely; but he succeeded somehow.

* For the broad distinction between the flint *flake*-tools and the flint *nodule*-tools I cannot do better than refer you to the excellent figures of them in the report of my lecture on Kent's Cavern, published by Messrs. Collins, Sons, & Co. (see figs. 1 to 4 and fig. 10).

You are aware that certain metals, such as gold and silver, are found uncombined with other things and are called "native." Copper is occasionally found native, and I have seen pieces in Cornwall in that state. In America a very large amount of native copper is found, but native iron is found nowhere except in the form of meteoric iron. Eminent Egyptologists inform me that the earliest known mention of "iron" occurs in an Egyptian papyrus, and that the word there used as the name of iron signifies "stone from heaven." Prior to the iron period man's knowledge of metals consisted of an acquaintance with copper and tin, which, united, make bronze. In order to do this he had to find his copper and tin, smelt them, and unite them in proper proportions to give them the proper degree of hardness. You are aware that in the Bible and other old books we meet with the word "brass." I believe it is safe to say that it should always be "bronze." Brass is a compound of copper and zinc, and all analyses show that the ancient so-called brasses were compounds of copper and tin, and were therefore bronzes. I have no doubt that the tin mentioned by Ezekiel and Homer, and other old writers, was Cornish tin—excuse that bit of nationality, for I am a Cornish man. The late Dr. George Smith, who has written so exhaustively on the source of the tin of antiquity, says: "We have ranged over the whole line of Egyptian maritime commerce, as given in its earliest authentic records, but find no trace of tin being carried from the east or south to that country. . . . We have found, indeed, that tin was known and recognised as an article of traffic, but that, instead of its coming from the east to Egypt, it has been invariably exported from Egypt to the east" (*The Cassiterides*, 1863, p. 23.) It appears, in short, that the tin now found so abundantly in the island of Banca, east of Sumatra, was not then worked or known, but that the ancient tin came from the Cassiterides—another name for Cornwall.

It is thought that during a short period prior to the bronze age man made copper tools. The period was probably but short, because the copper was not hard enough to form useful tools. Earlier still man made tools exclusively of bone and stone, during what has been termed the "Stone Period." The stone tools of pre-metallic times are readily

divisible into two groups—those which are and those which are not polished; and a study of the circumstances under which they are found shows that, *in any one and the same district,* they belong to distinct eras, the unpolished being the more ancient. Moreover, the unpolished tools are found, in Britain and Western Europe generally, commingled with remains of extinct mammals, which the polished ones never are. It was reserved for Sir John Lubbock to give to the two series, as well as to the deposits and periods to which they belong, the happy names of "Palæolithic" and "Neolithic;" the former term signifying *ancient stone* and the latter *new stone;* and the names were at once adopted by anthropologists generally.

It should be understood that whilst these archæological distinctions simply denote different states of civilization when applied to the world as a whole, they have a clear and trustworthy value in *relative* chronology when restricted, as in the present instance, to a limited and long settled region like Western Europe. Let us now proceed to the inquiry, What indications of these successive archæological periods are there in Kent's Hole? An answer will be found by comparing the third and fourth columns of the table—the last in the first division and the first in the second division—headed respectively "Implements" and "Archæological." It will be seen that in the ovine bed relics of the iron and bronze periods were certainly found, and that this deposit has possibly, but not certainly, yielded traces of the neolithic period. The doubt intended to be expressed by the note of interrogation after the word "and" springs out of the fact that the committee now conducting the researches in the cavern for the British Association have found no such traces, so that it seems probable that the neolithic period is entirely unrepresented in Kent's Hole. On the other hand, the Rev. J. MacEnery, who, as you know, explored the cavern fifty years ago, appears to have found one polished stone tool in the black mould; but even this specimen would scarcely be conclusive, as such tools may have been used for sacred or symbolical purposes long after the neolithic period had ceased.

Be this as it may, every tool below the black mould belongs to the palæolithic period. But this period, at least so far as

Kent's Cavern is concerned, must be divided into two, corresponding respectively with the hyenine and ursine deposits and eras. The least ancient of these is the period of flint-*flake* implements, and the most ancient is that of flint-*nodule* implements. The two series are never commingled; the distinction between them is broad, and, as must be remembered, they both belong to the times of extinct mammals.

If we try to get some notion of the amount of time represented here, let me ask you to bear in mind (and I have no political meaning in what I am going to say) that men are conservative in proportion as they are ignorant. What I mean may be illustrated thus—British people have been in the habit of eating British beef and mutton. If I tried to prevail on a household to eat Australian beef, I should succeed in the dining-room long before I could do so in the kitchen. In the same way, when man had nothing but rude, flint, unpolished tools, he may well be termed profoundly ignorant, and his emergence from that condition was no doubt extremely slow. He emerged from the neolithic period probably a little more rapidly, and from the bronze period more rapidly still; but it cannot be doubted, knowing what human nature is, that the periods described represent a prodigious amount of time. Further, there comes this question, which has a very important bearing on the subject before us, Were the neolithic men, the men who used polished tools, the descendants of the paleolithic men, or were they another race that came in and conquered them? The question is not capable of being answered; but it will be seen that, if they were a separate race, then we have two peoples having an ancestry removed far back into a still more remote antiquity. The same thing applies to all the other periods. Further, it must not be supposed that when the neolithic period began, the paleolithic tools were dropped at once and for ever. For rough and ready purposes a man would chip a flint into shape, and perform with it such work as it was capable of doing, without taking the trouble to polish the tool, although he did live in the neolithic age, and the same thing would probably be done in the bronze age. You must therefore be very careful how you use these terms. Some people seem to have had their minds perplexed about

the discoveries recently made in the Troad; and we are told that what is supposed to be the site of ancient Troy presents this anomalous state of things: a deposit containing highly wrought implements, having a deposit overlying it with more rudely wrought tools; as if men had retrograded. But is this really anomalous? There was a time when, before the arrival of the Romans in this country, the Britons were exceedingly rude, and the implements they left behind them represent a very rude age. Then came the Romans, and the deposits lying on the British remains contain Roman industrial products showing higher culture. Then the Romans withdrew, and the subsequent British remains showed something like retrogression, although the Britons had improved to some extent under the Romans. When the rude Saxons came the signs of retrogression were still much more strongly marked. That in all probability suggests the explanation of what is supposed to be a difficulty at Troy.

Directing attention once more to the table, you will see the words, "Danish Bog" heading the fifth column. "In the time of the Romans," says Sir Charles Lyell, "the Danish isles were covered, as now, with magnificent beech forests. Nowhere in the world does this tree flourish more luxuriantly than in Denmark, and eighteen centuries have done little or nothing towards modifying the character of the forest vegetation."* Throughout the country there are to be found a number of bogs, which have been made the subject of investigation by those eminent men, the Danish archæologists, and this is the outcome of their inquiries. The uppermost layer consists of debris of beeches and other trees. Whenever you reach a certain depth you cease to have any traces of beech; but, in their place, the "Pedunculated oak"—that is, an oak whose acorns have long stems or peduncles; lower still, there is another variety of oak in which the acorn has little or no "peduncle," and this variety is known as the "Sessile oak;" and lower still is the Scotch fir. The Scotch fir will not grow in Denmark now, even when coaxed and petted by the nurseryman. Here, then, is a chronological series, which it is possible to convert roughly into time. Remember that the luxuriant

* *Antiquity of Man*, 1873, p. 17.

beech in Denmark was a state of things existing 1800 years ago, and had not then just commenced. We cannot avoid going back at least a couple of hundred years before that, and saying, "Perhaps 2000 or more years ago the beech era commenced in Denmark." Moreover, the beech still holds possession of the country, and, left to itself, would, in all probability retain it for a further period of, say, 500 years; so that a moderate estimate gives 2500 years as the minimum value of the most recent term of the bog series; and there are four such terms. Were we sure that they were all of equal value we should get 10,000 years for the whole; but, to avoid all appearance of greed, let us be content with half that time, and take 5000 years as the chronological value of the entire bogs.

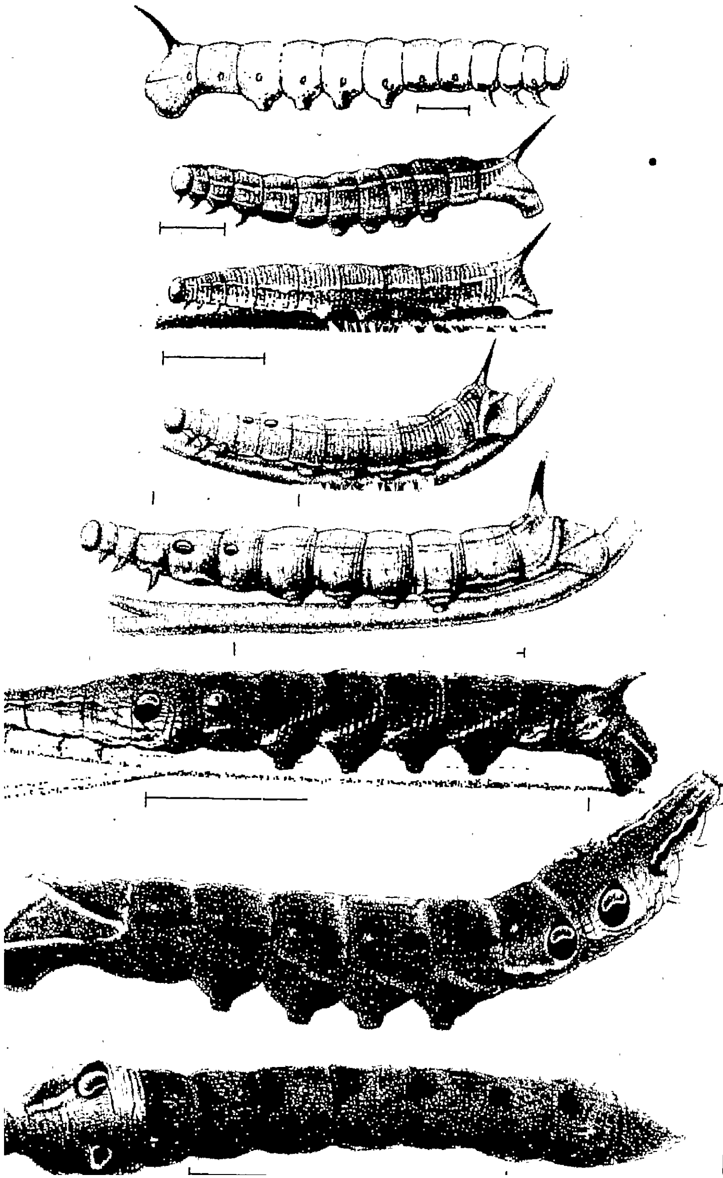
Have these successive forests disappeared through any great convulsion? Nothing of the kind; for the aspen and other trees are found from top to bottom. The changes that have occurred were potent, so far as the Scotch fir and then the oaks were concerned, but were not potent in the case of the entire vegetation, for the aspen has lived through them.

I want you now to co-ordinate the two columns in the table, headed "Archæological" and "Danish Bog." Industrial human remains are found in these bogs: iron implements extend just to the very bottom of the beech portion, but not under it. Below that, embracing the whole of the pedunculated oak period and about one-half of that of the sessile oak, are bronze tools. Underneath that there are no traces of metal of any kind, but polished stone implements are found. The eminent Steenstrup found, with his own hands, beneath a Scotch fir, at the bottom of the bog, a well-polished flint implement, showing that all these successive periods had not taken him back to the palæolithic age. It should be noted too, as being in harmony with this, that every one of the organic remains, found from top to bottom in the Danish bog, are remains of such species as still live. I quote from Sir Charles Lyell: "All the land and fresh-water shells, and all the mammalia, as well as the plants whose remains occur buried in the Danish bogs, are of recent species."—(*Antiquity of Man*, 1873, p. 7).

Let us now co-ordinate these bogs with Kent's Cavern,

We go back through the iron, bronze, and neolithic periods, and are thus taken so far back as the Scotch fir in the Danish bogs. Where are we in the cave? We have only got back to the bottom of the black mould. The 5000 years of the Danish bog takes us not a step further, if so far; and beyond that are the granular stalagmite, black band, cave earth, crystalline stalagmite, and breccia; each an important term in a series stretching into a remote antiquity; but even to the last are clear traces of man—not his bones—but the implements he made.

I have thus conducted you rapidly through three of the lines of evidence; and although each one has failed to tell us the exact number of years, I must call your attention to the fact that the three are entirely independent of one another; yet they all proclaim a high antiquity. If you have three men whose testimonies in a court of law are somewhat vague, yet if there be no collusion, and if there be a general consensus in their testimonies, you think that evidence very strong, and so you may do here. There are two important witnesses whose testimonies we have not heard—the geographical and the climatological. Time will not permit me to call them now. Let me ask you in conclusion to remember that we have been dealing with, not the antiquity of man, but the antiquity of man in Devonshire; and, unless Devonshire were the cradle of the human race, which is very improbable, this must fall very far short of the antiquity of man in the world.



ON

CERTAIN RELATIONS BETWEEN PLANTS AND INSECTS.

SIR JOHN LUBBOCK, Bart., M.P., F.R.S., D.C.L., delivered in the City Hall the fourth of this winter's Series of Lectures to the Glasgow Science Lectures Association. There was a good attendance of ladies and gentlemen. Mr. Charles Tennant of the Glen presided, and on the platform were also Sir William Thomson, Sir James Lumsden, Dr. Fergus, Professor Thomson, Messrs. Mirrlees, J. R. Napier, W. R. W. Smith, Andrew Paton, &c.

The Chairman, in introducing Sir John Lubbock to the meeting, said—That fortunately for him his duty on the present occasion was a simple and very formal one. He believed he merely had to mention the name of the distinguished lecturer to ensure cordial recognition at their hands. (Applause.) There were many ardent naturalists amongst the audience, who had followed with no small interest Sir John Lubbock's interesting investigations into the habits and mysteries of insect life, and who had perused and thoroughly enjoyed the many contributions he had made to the literature and their knowledge of this interesting subject. But it was not only to these smaller objects in nature that Sir John had devoted his attention. He had written two works which were very well known to the reading public, namely, his work upon *Pre-Historic Times*, and his work upon *The Origin of Civilisation*. These had placed

him in the first rank of archæologists, and had been translated into more than one language. His position as a scientific man was recognised as thoroughly in Germany and France as it was in America and in this country. But Sir John had other claims upon their consideration and gratitude. He had been for eight years a member of Parliament, and during that period he had passed many valuable Acts of Parliament. For four or five times he had introduced a bill "for the better preservation of our national monuments," and though it had three times passed the second reading, the measure still remained to be passed. The object of that bill was to preserve our national monuments, not so much from the ravages of time, as from the unnecessary ravages of man. The Chairman then called upon Sir John to deliver his lecture. (Applause.)

Sir JOHN LUBBOCK said,—When we examine any of the common wild flowers which grow around us in such exquisite beauty and endless variety, we find that in addition to the more striking differences of form, size, and colour, they differ also in many minor points; some, for instance, being hairy, some smooth, some sticky. Now, why is this? Why are some plants hairy, some glossy, and others sticky? In many of these cases we shall find that the peculiarities in question may be accounted for by the relations of plants to insects. Some of the most interesting of these have reference to the mode in which the pollen is carried by insects from flower to flower. In fact, it is not going too far to say, that we owe the beauty and fragrance of our flowers to the agency of insects, and especially of bees. The colours and odours attract the insects which visit the flowers for the sake of the honey and pollen. This is beneficial in two ways: firstly, they carry the pollen from plant to plant; and, secondly, this mode of transference effects a great economy of pollen. In plants when the pollen is carried by the wind, as for instance in the Scotch fir, by far the greater part is wasted. Hence they require a great deal more pollen than those plants, such as the white-dead nettle, in which it is carried from flower to flower by insects. Into this part of the question, however, I do not purpose to enter this evening. Of course, the most familiar relation between plants and insects is,

that the one affords in thousands of cases the nourishment necessary to the other. That plants afford food to thousands of insects is well known to every one. There are, however, cases in which the plants have turned the tables on the insects, and are in their turn the aggressors, being modified in such a manner as to enable them to capture and digest living insects. In the American genus Dionæa, the leaves have a joint in the middle, and when a fly or other insect alights on them they close up suddenly and capture it. Mr. Canby has recently tried them with various substances. Cheese he finds to disagree very much with them. In an English genus Drosera, the sundew, not uncommon in damp places, the hairs which cover the leaf fold over and capture insects. Another case is afforded by the Utricularia, a water plant, which is covered with small bags, formed on the principle of eel traps, which catch large numbers of small water creatures. The genus Sarracenia has some of the leaves in the form of a pitcher, which secretes a fluid, and is lined internally with hairs pointing downwards. Up the outside of the pitcher there is a line of honey glands, which lure the insects to their destruction. Bees, however, appear to be scarcely ever caught. Flies and other insects which fall into this pitcher cannot get out again, and are actually digested by the plant. The relations between ants and plants are in many respects very curious and interesting. The honey in flowers is no doubt intended to attract insects, and especially bees, with the view of securing the transfer of the pollen from one flower to another. At first sight it may seem to be an objection to this, that many plants secrete honey on other parts beside the flowers; and this is, perhaps, one of the reasons why the earlier botanists missed the true explanation.

Belt and Delpino have, I think, suggested the true function of these extra floral nectaries. The former of these excellent observers describes a South American species of Acacia, which, if unprotected, is apt to be stripped of its leaves by a leaf-cutting ant, which uses the leaves not directly for food, but, according to Mr. Belt, to grow mushrooms on. The Acacia, however, bears hollow thorns, and each leaflet produces honey in a centre-formed gland at the

base, and a small sweet pear-shaped body at the tip. I consequence, it is inhabited by myriads of a small ant whic nests in the hollow thorns, and thus finds meat, drink, an lodging all provided for it. These ants are continuall roaming over the plant, and constitute a most efficient body guard, not only driving off the leaf-cutting ants, but eve in Mr. Belt's opinion, rendering the leaves less liable to b eaten by herbivorous mammalia. I am not aware that an of our English plants are protected in this manner fror browsing quadrupeds; but not the less do our ants perforn for them a very similar function, by keeping down th number of small insects which would otherwise rob them o their sap, and strip them of their leaves. M. Forel watchec from this point of view a nest of *Formica pratensis*. H found that the ants brought in dead insects, small cater pillars, grasshoppers, cercopis, &c., at the rate of abou twenty-eight a minute, or more than 1600 in an hour.

When it is considered that the ants work not only all day, but in warm weather often all night too, it is easy to see how important a function they fulfil in keeping down the numbers of small insects. Some of the most mischievous indeed, certain species, for instance, of Aphides and Coccus, have turned the tables on the plants, and converted the ants from enemies into friends, by themselves developing nectaries and secreting honey, which the ants love. We have all seen the little brown garden ant, for instance, assiduously running up the stems of plants to milk their curious little cattle. By this ingenious idea not only do the Aphides and Cocci secure immunity from the attack of the ants, but even turn them from foes into friends. They are subject to the attacks of a species of ichneumon, which lays its eggs in them; and Delpino has seen the ants watching over them with truly maternal vigilance, and drawing off the ichneumons whenever they attempted to approach.

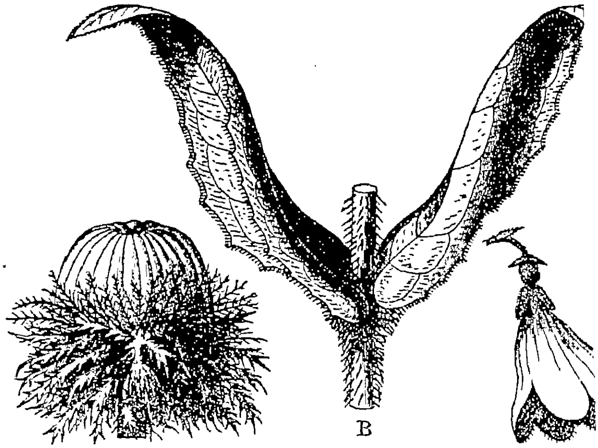
But though ants are in some respects very useful to plants, they are not wanted in the flowers. The great object is to secure cross fertilisation, but for this purpose winged insects are almost necessary, because they fly readily from one plant to another, and generally confine themselves for a certain time to the same species.

Creeping insects, on the other hand, naturally would pass from each flower to another on the same plant; and as Mr. Darwin has shown in his last work, it is an advantage that the pollen should be brought from a different plant altogether; moreover, when ants quit a plant they would naturally creep up another close by, without any regard to species.

Hence, even to small flowers, such as many Cruciferæ, Compositæ, Saxifrages, &c., which, as far as size is concerned, might well be fertilised by ants, the visits of flying insects are much more advantageous. Moreover, if larger flowers were visited by ants, not only would they deprive the flowers of their honey without fulfilling any useful function in return, but they would probably prevent the really useful visits of bees.

If you touch an ant with a needle or a bristle, she is almost sure to seize it in her jaws; and if bees, when visiting any particular plant, were liable to have the delicate tip of their proboscis seized on by the horny jaws of an ant, we may be sure that such a species of plant would soon cease to be visited. On the other hand, we know how fond ants are of honey, and how zealously and unremittingly they search for food. How is it, then, that they do not anticipate the bees and secure the honey for themselves? Kerner has recently published a most interesting memoir on this subject, and pointed out a number of ingenious contrivances by which flowers protect themselves from the unwelcome visits of such intruders. The most frequent are by the interposition of *chevaux de frise*, which ants cannot penetrate, glutinous parts which they cannot traverse, slippery slopes which they cannot climb, or barriers which close the way. Firstly, then, as regards *chevaux de frise*. In some respects they are the most effectual protection, since they exclude not only creeping insects, but also other creatures, such as slugs. With this object it will be observed that the hairs which cover the stalks of so many herbs usually point downwards. A good example of this is afforded, for instance, by a plant allied to our common blue scabious, *Knautia dipsacifolia*, A. The heads of the common Carlin (*Carlina vulgaris*, B) again present a sort of thicket which must offer an impenetrable

barrier to ants. Some species of plants are quite sı except just below the flowers. The same conside explains the large number of plants which are more sticky—a condition generally produced, as for insta the flowers of the gooseberry and of *Linnea borealis*, C, presence of glandular hairs. Kerner has called attent an interesting case afforded by one of our common plants—*Polygonum amphibium*, which, as its name de grows sometimes in water, sometimes on land. So l



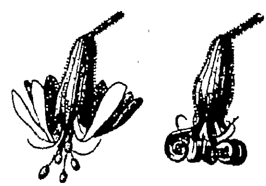
B

A

C

it grows in water, it is protected by the water, and its is smooth; but, on the other hand, those specimens w live on land throw out certain hairs, which terminat sticky glands, and thus prevent small insects from cree up to the flowers. In this case, therefore, the plant is sticky, except just when this condition is useful. All t viscous plants, so far as I know, have upright or horizc flowers. On the other hand, where the same objec effected by slippery surfaces, the flowers are often pendul creeping creatures being thus kept out of them, just as

pendulous nests of the weaver bird are a protection from snakes and other enemies. As instances of this kind, I may mention the common snowdrop, and the cyclamen, though no doubt the pendulous habit is also useful in excluding rain. The last device for the exclusion of ants and other creeping insects to which I shall allude, is that of barriers, which either leave too small a space for the entry even of an ant, or which are entirely closed. Various instances of the latter will suggest themselves to every one. Take the common foxglove, for example. It is a closed box, within which are the anthers, the pistil, and the honey. It has the specialities of a flower which is adapted for cross fertilisation by insect agency, namely, colour, honey, and the arrangement of the stamens and pistil. But it is closed. At first sight this may appear an anomaly and a disadvantage, but the contrary is the case. The flower is adapted for fertilisation by humble bees, and they alone can force open the door. To other insects the box is closed, and thus the flower is protected from robbery. I have elsewhere suggested that the so-called "sleep" of flowers had reference to the habits of insects, on the ground, that flowers which are fertilised by night-flying insects would derive no advantage by being open in the day, while, on the other hand, those which are fertilised by bees would gain nothing by being open at night. I confess that I suggested this with much diffidence, but it may now, I think, be regarded as well established. *Silene nutans*, the Nottingham catchfly, is a very instructive species from this point of view, and indeed illustrates a number of interesting points in the relations between plants and insects. Its life history has recently been well described by Kerner. The upper part of the flowering stem is viscid, from which it has derived its local name — the Nottingham catchfly. This prevents the access of ants and other small creeping insects. Each flower lasts three days, or rather three nights. The stamens are ten in number,



arranged in two sets, the one set standing in front of the sepals, the other in front of the petals. Like other night flowers, it is white, and opens towards evening, when it also becomes extremely fragrant. The first evening, towards dusk, the stamens in front of the sepals grow very rapidly for about two hours, so that they emerge from the flower; the pollen ripens and is exposed by the bursting of the anther. So the flower remains through the night, very attractive to and much visited by moths. Towards three in the morning the scent ceases, the anthers begin to shrivel up or drop off, the filaments turn themselves outwards, so as to be out of the way, while the petals on the contrary begin to roll themselves up, so that by daylight they close the aperture of the flower and present only their brownish green undersides to view, which, moreover, are thrown into numerous wrinkles. Thus by the morning's light the flower has all the appearance of being faded. It has no smell, and the honey is covered over by the petals. So it remains all day. Towards evening, however, everything is changed. The petals unfold themselves by eight o'clock; the flower is as fragrant as before; the second set of stamens have rapidly grown, their anthers are open, and the pollen again exposed. By morning the plant is again asleep. the anthers shrivelled, the scent ceased, and the petals rolled up as before. The third evening again the same process takes place, but this time it is the pistil which grows, and the long spiral stigmas on the third evening take the position which on the previous two had been occupied by the anthers, and can hardly fail to be dusted by the moths with pollen brought from another flower.

An objection to the view that the sleep of flowers is regulated by the visits of insects might be derived from the cases of those flowers which close early in the day, the well-known *Tragopogon pratense,* or "John-go-to-bed-at-noon," for instance; still more, such species as *Lapsana communus* or *Crepis pulchra,* which open before six and close before ten in the morning. Bees, however, are very early risers, while ants come out much later, when the dew is off the grass, so that it might well be an advantage to a flower which was quite unprotected, to open early for the bees, and close again

before the ants were out, thus preserving its honey for another day.

So much for the first part of my subject. I must now pass to my second—the action of plants on insects, and I will take especially the case of caterpillars. I will not, however, refer to isolated cases, however interesting in themselves, on the present occasion, but will take a group, and see how far we can explain its various colours and markings, and what are the lessons which they teach us. For this purpose, I think I cannot do better than select the larvæ of the Sphingidæ, which have just been the subject of a masterly monograph by Dr. Weissmann, the learned professor of Freiburg. Let me ask you then to glance at the diagrams of caterpillars behind me: they are very different in colour, green, white, yellow, brown, sometimes even gaudy, varied with spots and patches, streaks and lines. Now, are these merely casual and accidental, or have they a meaning and a purpose?

In many, perhaps in most cases, the markings serve for the purpose of concealment. When, indeed, we see caterpillars represented on a white sheet of paper, or if we put them on a plain table and focus the eye on them, the colours and markings would seem, if possible, to render them even more conspicuous, as for instance, in the diagram of *D. galii;* but amongst the intricate lines and varied colours of foliage and flowers, and if the insect be a little out of focus, the effect is very different.

Let us begin with *Chœrochampa elpenor*, the Elephant hawkmoth. The caterpillars, as represented in most entomological works, are of two varieties, most of them brown, but some of them green. Both have a white line on the three first segments, two remarkable eye-like spots, the fourth and fifth a very faint medium line, and another more than four inches long. I will direct your attention specially to three points. What mean the eye-spots and the faint lateral line? and why are some of the caterpillars green and some brown, offering thus such a marked contrast to the leaves of the *Epilobium parvum* on which they feed? Other questions will suggest themselves later, for I must call your attention to the fact, that when they first quit the egg, and

come into the world, they are quite different in ap
being like so many other small caterpillars, bright g
almost exactly the colour of the leaves on which t
That this colour is not a necessary or direct conse
their food, we see from the case of quadrupeds, wh
need not say, are never green. This tint, howev
obviously a protection to them, that the explanatio
green colour of small caterpillars suggests itself to ev
After five or six days, and when they are about a
of an inch in length, they go through their first
In this second stage they have a white sub-dor
stretching along the body, from the horn to the
and after a few days, but not at first, traces of t
spots appear on the fourth and fifth segments. T
also a second pale line running along the side.
another five or six days, and when about half an
length, our caterpillars moult again. In this thir
the commencement of the eye-spots is more marked,
on the contrary, the lower longitudinal line has disap
After another moult the eye-spots are still more distin
white gradually becomes surrounded by a black line
the centre becomes somewhat violet. The sub-dors
has almost or entirely disappeared, and in some spe
faint diagonal lines make their appearance. Som
assume a brownish tint, but not many. A fourth
takes place in seven or eight days, and when the cate
are about an inch and a half in length; now the diff
shows itself still more between the two varieties, so
maining green, while the majority become brown. T
spots are more marked, and the pupil more distin
diagonal lines plainer, while the sub-dorsal line is onl
cated on the first three and the eleventh segments.
last stage has already been described. (See *Frontispi*

Now, the principal points to which I desire to draw
attention are, first, the green colour; second, the lo
dinal lines; third, the diagonal lines; fourth, the
colour; and fifth, the eye-spots. There are, however
other very instructive points to which I should like to
your attention presently, because they throw much lig
this group of insects. But to return to my five points

regards the first, the green colour. I think I need say no more. The value to the young insect, the protection it affords, are obvious. We must all have observed how difficult it is to distinguish small green caterpillars from the leaves on which they feed. When, however, they become somewhat larger, their form betrays them, and it is important that there should be certain marks to direct the eye from the outlines of the body. This is effected, and much protection given, by longitudinal lines, such as those occurring in the second stage of our larvæ. These lines, both in colour and thickness, resemble some of the lines on leaves (especially those, for instance, of grasses), and also the streaks of shadow which occur among foliage. If, however, this is the explanation of these, then they ought to be wanting, as a general rule, in very small caterpillars, and to prevail most among those which feed on or among grasses.

Similar lines occur in a great number of caterpillars belonging to most different groups of butterflies and moths, as you may see by turning over the illustrations of any monograph of the Lepidoptera. We have seen that they exist among the hawkmoths, as, for instance, in *Ch. elpenor.* They occur in many butterflies, as, for instance, in Arge-galathea, which feeds on the cats'-tail grass, and among moths, as, for instance, in *Pyrophila tragopoginis*, which feeds on the stems of the John-go-to-bed-at-noon (Tragopogon); now you will find that the smallest caterpillars rarely possess these white streaks. As regards the second point also, the streaks are generally wanting in caterpillars which feed on large leaved plants. The Satyridæ, on the contrary, all possess them, and all live on grass. In fact, we may say, as a general rule, that these longitudinal streaks only occur on caterpillars which live on or among narrow-leaved plants. We have seen that in a later stage these lines disappear on certain segments, and are replaced by diagonal lines. In this particular species these diagonal lines are faint, but in a great many other caterpillars belonging to the most distinct families of butterflies and moths they are conspicuous, and no doubt important. Now, these diagonal lines come off just at the angle of the ribs of leaves, and resemble them very much in general effect. They are

present also especially in species which feed on large leav plants; and, I believe, I may say, that though a great ma species of caterpillars are thus marked, these lines are rarel if ever, present in species which live on grass. In this di gram are represented three of such caterpillars, one belongi to each of the three great divisions of Lepidoptera—namel that of the purple emperor (*Apatura iris*), which feeds on t oak; as representing the butterflies, that of the privet haw moth; and lastly, that of a moth, the Kentish glory (*Endr mis versicolor*). It might at first be objected to this vie that there are many cases, as, indeed, in our elephant haw moth, in which caterpillars have both longitudinal ar diagonal lines. A little consideration, however, will expla this. In small caterpillars these oblique lines would be us less, because they must have some relation, not only colour, but in their distances apart, to the ribs of the leave Hence, while there are a great many species which ha longitudinal lines when young, and diagonal ones when the are older and larger, there is not, I believe, a single or which begins with diagonal lines and then replaces the with longitudinal ones. You will also observe that the long tudinal lines in our caterpillar have disappeared exactly o those segments which have no diagonal ones. This also ofte occurs, and is striking where the lines are conspicuous. I is an advantage, because white lines crossing one another a such an angle would have no relation to anything whic occurs in plants, and would make the creature more consp cuous. It is beneficial, therefore, that when the diagona lines are developed, the longitudinal ones should disappea

There is one other point in connection with these dia gonal lines to which I must call your attention. In ou species they are white, but in some cases, as for instance in the beautiful green caterpillars of the privet hawk moth, the white streak is accompanied by a coloured one in that case purple. At first we might think that thi would be a disadvantage, as tending to make the caterpilla more conspicuous, and in fact, if we put one in full view, ou for instance on a table, the coloured lines are very striking But we must remember that the habit of the insect is to si on the underside of the leaf, generally near the midrib, and

in the subdued light of such a situation the coloured lines beautifully simulate a line of soft shadow, such as must always accompany a strong rib or the edge of a leaf, and I need not tell any artists that the shadows of yellowish green must be purplish. Moreover, any one who has ever found one of these large caterpillars will, I am sure, agree with me that it is surprising, when we consider their size and conspicuous colouring, how difficult they are to see.

The next point is the colour of the mature caterpillars. We have seen that some are green and others brown, and the green ones are obviously merely those which have retained their original colour. Now for the brown colour. It is evident that this makes the caterpillars even more conspicuous among the green leaves than would otherwise be the case. Let us see, then, whether the habits of the insects will throw any light upon the riddle. What would you do if you were a big caterpillar? Why, like most other defenceless creatures, you would feed by night and lie concealed by day. So do these caterpillars. When the morning light comes they creep down the stem of the foot plant, and lie concealed among the thick herbage and dry sticks and leaves near the ground, and it is obvious that under such circumstances the brown colour really becomes a protection. It might, indeed, be said that the caterpillars having become brown, concealed themselves on the ground—that, in fact, we were reversing the state of things. But this is not so, because while we may say, as a general rule, that with some exceptions, due to obvious causes, large caterpillars feed by night and lie concealed by day; it is by no means always the case that they are brown, some of them still retaining the green colour. We may then conclude that the habit of concealing themselves by day comes first, and that the brown colour is a later adaptation. It is, moreover, interesting, that while the caterpillars which live on plants often go down to the ground and turn brown, those which feed on trees or large plants remain on the underside of the leaves, and retain their green colour. Thus, in *Smerinthus ocellatus*, which feeds on the willow and sallow, *S. populi*, which feeds on the poplar, and *S. tiliæ*, which frequents the lime, the caterpillars all remain green; while in the Convolvulus

hawkmoth, which frequents convolvulus, *Ch. nerii*, which feeds in this country on the periwinkle, *Ch. celerio*, *Ch. elpenor*, and *Ch. procellus*, which feed on galium, most of the caterpillars turn brown. There are, indeed, some caterpillars which are brown, and which yet do not go down to the ground. These caterpillars, however, place themselves in peculiar attitudes, which, combined with their brown colour, make them look almost exactly like bits of stick or dead twigs.

The last of the five points to which I called your attention was the eye-spots. In some cases spots may serve for concealment, by resembling the marks on dead leaves. In *Deilephila hippophaœ*, which feeds on the hippophaæ, or sea buckthorn, a very grey-green plant, the caterpillar also is a very similar grey-green, and has, when full grown, a single red spot on each side, which, as Weissmann suggests, at first sight much resembles in colour and size the berries of hippophaæ, which, moreover, are present, though not ripe at the same period of the year.

Again, in *Chærocampa tersa*, there is an eye-spot on each segment, which mimics the flower of the plant on which it feeds (*Spermacoce hyssopifolia*). White spots in some cases also resemble the spots of light which penetrate the foliage. In others, however, and at any rate in our elephant hawkmoth, the eye-spots certainly render the insect more conspicuous. Now, in some cases, as Wallace has pointed out, this is an advantage, rather than a drawback. Suppose that from the nature of its food, or any other cause, as for instance from being covered with hairs, a small green caterpillar were very bitter, or in any other way disagreeable or dangerous, still, in the number of small green caterpillars which birds it would be continually swallowed by mistake. If, on the r hand, it had a conspicuous and peculiar colour, its evil would serve to protect it, because the birds would soon gnise and avoid it, as Weir and others have proved exnentally. We have a striking case of this, amongst the moths, in *Deilephila euphorbiæ*, which, feeding on the orbia, with its bitter milky juice, is very distasteful to and is thus actually protected by its bold and striking rs. The spots on our elephant hawkmoth caterpillar

do not admit of this explanation, because the insect is quite good to eat—I mean for birds. We must therefore, if possible, account for them in some other way. There can, however, I think, be little doubt that Weissmann is right when he suggests that they actually protect the caterpillar by frightening its foes.

Every one must have observed that these large caterpillars have a sort of uncanny, poisonous appearance, that they suggest a small thick snake or other evil being; and the eyes do much to increase the deception. Moreover, the segment on which the eyes are placed is swollen, and the insect, when in danger, has the habit of retracting its head and front segments, which gives it an additional resemblance to some small reptile. That small birds are, as a matter of fact, afraid of these caterpillars (which, however, I need not say are in reality altogether harmless), Weissmann has proved by actual experiment. He put a caterpillar in a tray in which he was accustomed to place seed for birds. Soon a little flock of sparrows and other small birds assembled to feed as usual. One of them lit on the edge of this tray, and was just going to hop in, when she spied the caterpillar. Immediately she began bobbing her head up and down, but was afraid to go nearer. Another joined her, and then another, until at last there was a little company of ten or twelve birds, all looking on in astonishment, but not one ventured into the tray, while one which lit in it unsuspectingly beat a hasty retreat, in evident alarm, as soon as she perceived the caterpillar. After watching for some time, Weissmann removed the caterpillar, when the birds soon attacked the seeds. Other caterpillars also, probably, as for instance, that of *Deilephila nicæa*, are protected by their resemblance to spotted snakes, which may throw some light on the curious fact that the caterpillars of some of the foreign hawkmoths have acquired the power of hissing. There are many other points connected with the colouring of Sphinx caterpillars to which I might refer, if time permitted. I will only allude to two. The peculiar hues of the death's-head hawkmoth caterpillar, which feeds on the potato, and unites so beautifully the brown of the earth, the yellow and green of the leaves, and the blue of the flowers, that in spite

of its size it can scarcely be perceived unless the eye focused exactly upon it. The other is the Anaryx. T caterpillars of this genus differ in style of colouring from other Sphinx larvæ, having longitudinal bands of brown a green. Why is this? Their habitat is different. Th feed on the leaves of pinaster, and their peculiar colouri offers a general similarity to the brown twigs and narr green leaves of a Conifer. There are not many species Lepidoptera which feed on the pine, but there are a few, a I have here diagrams of two, *Achatia spreta* and *Dendrobi pini*, both of which, as you will see, have a very analogc style of colouring, while the latter has also tufts of blui green hairs, which singularly mimic the leaves of the pi I have added also the larvæ of a species of sawfly belonging the Hymenoptera, and you will see that here also the colouri is curiously similar. But, as Weissmann points out, we m learn another very interesting lesson from these caterpilla They leave the egg, as we have seen, a plain green, like many other caterpillars, and gradually acquire a successi of marking, the utility of which I have just attempted explain. The young larvæ, in fact, represents an old for and the species in the lapse of ages has gone through t stages which each individual now passes through in a fe weeks. Thus the caterpillar of *Chærocampa procellus*, t small elephant hawkmoth, a species very nearly allied *Chærocampa elpenor*, passes through almost exactly t same stages as that of *Chærocampa elpenor;* but it leav the egg with a sub-dorsal line. No one can dout however, that there was a time when the new-born cate pillars of *Chærocampa procellus* were plain green, like tho of *Chærocampa elpenor*. In this respect, then, *Chærocamp procellus* is a newer specific form than *Chærocampa elpenc* Again, if we compare the mature caterpillars of Chærocamp we shall find that there are some forms, such as *Chærocamp myron* and *Chærocampa chærilus*, which never develop ey spots, but correspond to the second stage of *Chærocamp elpenor*. Here, then, we seem to have a species still in tl stage which *Chærocampa elpenor* must have passed throug long ago. The genus Deilephila, of which we have i England three species—the euphorbia hawkmoth, the galiu

hawkmoth, and the rayed hawkmoth—is also very instructive. The caterpillar of the euphorbia hawkmoth begins life of a clear green colour, without a trace of the subsequent markings. After the first moult, however, it has a number of black patches, a white line, and a series of white dots, and has, therefore, at a bound acquired characters which in *Ch. elpenor*, as we have seen, were only very gradually assumed. In the third stage the line has disappeared, leaving the white spots. In the fourth, the caterpillars have become very variable, but are generally much darker than before, and have a number of white dots under the spots. In the fifth stage there is a second row of white spots under the first. The caterpillars not being good to eat, there is, as we have already pointed out, no need for or attempt at concealment. Now, if we compare the mature caterpillars of other species of the genus, we shall find that they represent phases in the development of euphorbia. *D. hippophacæ*, for instance, is a plain green, with only a trace of the line, and corresponds therefore with a very early stage of *D. euphorbiæ*. *D. Zyogophylli* of South Russia has the line, and represents the second stage of *D. euphorbiæ*. *D. Livornica* has the line and row of spots, and represents equally the third stage. Lastly, *D. vespertilio*, and *D. galii*, have progressed further, and lost the longitudinal line, but they never acquire the second row of spots which characterises the last stage of *D. euphorbiæ*. They teach us, indeed, many instructive lessons. It would, in fact, be a great mistake to regard them as merely preparatory stages in the development of the perfect insect. They are much more than this, for the external circumstances act on the larvæ, as well as on the perfect insects, and both, therefore, are liable to adaptation. In fact, the modifications which insect larvæ undergo may be divided into two kinds—developmental, or those which tend to approximation to the mature form; and adaptational or adaptive, those which tend to suit it to its own mode of life. Thus in some cases very similar larvæ produce very dissimilar perfect insects. In others similar, or comparatively similar perfect insects, have very dissimilar larvæ. Indeed, a classification of insects founded on larvæ would be quite different from that founded on the

perfect insects. The Hymenoptera, for instance, which, so far as the perfect insects are concerned, form a very homogeneous group, would be divided into two; or rather, one portion of them, namely, that of the sawflies, would be united to the butterflies and moths. Now, why do the larvæ of sawflies differ from those of the Hymenoptera, and resemble those of butterflies and moths? It is because their habits differ from those of the Hymenoptera, and they feed on leaves like ordinary caterpillars.

From this point of view, the transformations of the genus Sitaris, which have been very carefully investigated by M. Fabre, are peculiarly interesting. The genus Sitaris, a small beetle allied to Cantharis (the blister fly), and to Meloe (the oil beetle), is parasitic on a kind of bee (Anthophora), which excavates subterraneous galleries, each leading to a cell. The eggs of the Sitaris, which are deposited at the entrance of the galleries, are hatched at the end of September or beginning of October; and M. Fabre, not unnaturally, expected that the young larvæ, which are active little creatures, with six serviceable legs, would at once eat their way into the cells of the Anthophora. No such thing. Till the month of April following they remain without leaving their birthplace, and consequently without food; nor do they in this long time change either in form or size. M. Fabre ascertained this not only by examining the burrows of the Anthophoras, but also by direct observation of some young larvæ kept in captivity. In April, however, his captives at last broke from their long lethargy, and hurried anxiously about their prison. Naturally inferring that they were in search of food, M. Fabre supposed that this would consist either of the larvæ or pupæ of Anthophora, or of the honey with which it stores its cells. All three were tried without success. The first two were neglected, and the larvæ, when placed on the latter, either hurried away or perished in the attempt, being evidently unable to deal with the sticky substance. M. Fabre was in despair. "Jamais expérience," he says, "n'a éprouvé pareille déconfiture. Larves, nymphes, cellules, miel, je vous ai tous offert, que voulez vous donc, bestioles maudites?" The first ray of light came to him from our countryman Newport, who ascertained that a small para-

site found by Léon Dufour on one of the wild bees, and named by him Triungulinus, was in fact the larva of Meloe. The larvæ of Sitaris much resembled Dufour's Triungulinus. Acting on this hint M. Fabre examined many specimens of the Anthophoras, and found on them at last the larvæ of his Sitaris.

The males of Anthophora emerge from the pupæ sooner than the females; and M. Fabre ascertained that as they come out of their galleries the little Sitaris larvæ fasten upon them, not, however, for long. Instinct teaches them that they are not yet in the straight path of development, and watching their opportunity, they pass from the male to the female.

Being guided by these indications, M. Fabre examined several cells of the Anthophoras. In some the egg of the Anthophora floated by itself on the surface of the honey; in others, on the egg, as on a raft, sat the still more minute larvæ of the Sitaris. The mystery was solved. At the moment when the egg is laid, the Sitaris larva springs upon it. Even while the poor mother is carefully fastening up her cell, her mortal enemy is beginning to devour her offspring, for the egg of the Anthophora serves not only as a raft, but as a repast. The honey which is enough for either would be too little for both, and the Sitaris, therefore, at its first meal, relieves itself from its only rival. After eight days the egg is consumed, and on the empty shell the Sitaris undergoes its first transformation, and makes its appearance in a different form, as shown in Figure 10. The honey which was fatal before is now necessary; the activity which was before necessary is now useless; consequently, with the change of skin, the active slim larva changes into a white fleshy grub, so organised as to float upon the surface of the honey with the mouth beneath, and the spiracles above the surface. "Grâce a l'embonpoint du ventre, la larve est à l'abri de l'asphyxie," says M. Fabre. In this state it remains until the honey is consumed, then the animal contracts and detaches itself from its skin, within which the further transformations take place.

In the second stage, which M. Fabre calls the pseudo chrysalis, the larva has a solid corneous envelope, and oval

shape, and in its colour, consistency, and immobility reminds one of a dipterous pupa. The time passed in thi condition varies much. When it has elapsed, the anima moults again, changes its form, and assumes that shown i Figure 12. After this it becomes the pupa, Figure 13, with out any remarkable peculiarities. Finally, after these won derful changes and adventures, in the month of August th perfect Sitaris makes its appearance.

For such an inquiry as this the larvæ of Lepidoptera ar perfectly suitable, because they live an exposed life; becaus different species, even of the same genus, often feed on dif ferent plants, and are therefore exposed to different condi tions; and last, and not least, because we know more abou the larvæ of the Lepidoptera than of any other insect. Th larvæ of ants all live in the nest. They are fed by the per fect ants, and being therefore all subject to very similar con ditions, are all very much alike. It would puzzle even a good naturalist to determine the species of an ant larva while, as we know, the caterpillars of butterflies and moth are as easy to distinguish as butterflies and moths, nay, in some cases even more so.

For this purpose I have tabulated the larvæ of the British butterflies and of the larger moths, amounting to about 150 species.

Out of these 150, nearly forty, or, in round numbers, about one-fourth, are decidedly hairy; twenty-six, or about one-sixth, are black, or marked with black; seventeen, or about one-ninth, are marked with red; and eight, or about one-twentieth, are marked with blue. Of the remainder, the great majority are green. Now, of the twenty-six species which are black, or more or less marked with black, two are nasty, and consequently not eaten by birds; two live inside trees, and are inaccessible to birds. This leaves twenty-two, and out of these, no less than twenty are hairy. Of the seventeen species more or less marked with red, four are nasty; in five the lines are shadow lines, leaving six, all of which are hairy. Of the eight species which are more or less marked with blue, one has an unpleasant taste; two feed on plants with blue flowers; in one the blue mimics an eye; leaving four, all of which are hairy.

Now, if we look at the hairy caterpillars, thirty-seven in number, we shall find that twenty-one are more or less black, eleven brown, and five more or less marked with red or blue, or both ; not one of them is of the usual green colour.

There are, I may say in conclusion, five principal types of colouring among caterpillars. Those which live inside wood, or leaves, or underground, are generally of an uniform pale hue ; the small leaf-eating caterpillars are green, like the leaves on which they feed. The other three types may, *si parva licet componere magnis*, be compared with the three types of colouring among cats. There are the ground cats, such as the lion and puma, which are brownish or sand-colour, like the open places they frequent. So, also, caterpillars, which conceal themselves by day at the roots of their food-plant, tend, as we have seen, even if originally green, to assume the colour of earth. The spotted or eyed cats, such as the leopard, live among trees ; and their peculiar colouring renders them less conspicuous, by mimicking the spots of light which penetrate through foliage. These correspond to such caterpillars as are shown on the diagram. Lastly, there are the jungle cats, of which the tiger is the typical species, and which have stripes, rendering them very difficult to see among the brown grass which they frequent. It may, perhaps, be said that this comparison fails, because the stripes of tigers are perpendicular, while those of caterpillars are either longitudinal or oblique. This, however, so far from constituting a real difference, confirms the explanation, because in each case the direction of the lines follows those of the foliage. The tiger, which walks horizontally on the ground, has transverse bars ; the caterpillar, which clings to the grass in a vertical position, has longitudinal lines ; while those which live on large-veined leaves have oblique lines, like the oblique ribs of the leaves. Thus, then, I think we see reasons for many, at any rate, of those variations of colour and markings in caterpillars which at first sight seem so fantastic and inexplicable. I should, however, produce an impression very different from that which I wish to convey, were I to lead you to suppose that all these varieties have been explained or are understood. Far from it, they still offer a large and interesting field for study. Never-

theless, I venture to hope the evidence brought before you to-day, however imperfectly, is at least sufficient to justify the conclusion that there is not a hair or a line, not a spot or a colour, for which there is not a reason, which has not a purpose and a function in the economy of nature. What this purpose is, what lesson they teach, I have endeavoured in a few cases to indicate.

There are many practically, indeed endless, other examples which I might have brought before you; but there are far more questions which you might ask, and to which no one as yet could, I think, give a satisfactory reply. What, for instance, has regulated the sizes and forms, shades and colours, of the leaves of the beech, oak, and other trees? Why has the holly glossy leaves? What benefit does the fern derive from its honey glands? The researches of those eminent naturalists to whom I have referred—of Sprengel and Delpino, Hermann Müller, Weissmann, and, above all, of our illustrious countryman, Mr. Darwin, are of great interest from the questions they have solved, but perhaps of even greater importance from the lines of thought which they have opened up, and the new problems they have suggested.

On the motion of Sir James Lumsden a cordial vote of thanks was awarded to Sir John Lubbock, and the proceedings concluded with a similar compliment being paid to the Chairman.

STRUCTURE AND DEVELOPMENT OF THE BRAIN.*

WHEN I was invited to deliver a lecture under the auspices of this Association, it occurred to me that there was no topic taken from the science of Anatomy more likely to interest the audience than an account of the progress recently made in the investigation of the structure and development of the brain—as the organ in which all those vital processes are carried on by which we are brought into relation with the world around us—the centre to which all impressions from without are conveyed so as to give rise to our feelings or sensations—the source of origin of the impulses which cause our various active movements—and the seat during life of our intelligence or mind, that is, of our consciousness and memory, our feelings, emotions and passions, our will, our thoughts, judgment and reasoning power.

In adopting the title of "Evolution of the Brain," it appeared to me that I could best lay before you the groundwork upon which our views as to the relation of the structure and functions of the organ must mainly rest, by calling your attention to the principal facts which have been ascertained in the following divisions of our subject:—

1. The origin, early formation, and development of the brain in the embryo.

* This Lecture, when delivered, was illustrated very fully by numerous large diagrams, drawings, and specimens; but as it has been found impossible satisfactorily to reproduce these in figures within reasonable limits in this publication, it has been deemed expedient to omit them entirely, trusting that the statement in the text may be sufficient to indicate the Lecturer's meaning. The reader is reminded that the Lecture was delivered so long ago as in February 1877.

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2. The differences in its form, size, and structure a: animals.

3. A comparison of the brain of man with that o: animals most nearly resembling him, such as the anth morphous apes; with some remarks on the varieties of and size observed in the brain among different indivi and races of man.

4, and lastly. An attempt to form a scheme or pl: what may be called the architecture and mechanism o: brain, or, in other words, to trace the relation and coı tion of its various parts, not merely by the study of arrangement of the larger and more obvious masses, but by the microscopical investigation of its minute struc which makes us acquainted with the organised nature o texture and the properties of the materials employed i construction.

Such a view of the structure or anatomy of the l would naturally be accompanied or followed by an acc of its functions, but, as may be readily understood, so e sive and interesting a series of topics could not with ad tage be discussed in the present lecture, which I shall re the less, that I believe my colleague Dr. M'Kendrick more ably undertake this difficult task in the next wi course. At present therefore I shall not attempt to do r than to show the bearing which ascertained facts in anatomy have upon the knowledge and investigation of functions. Before proceeding with the special subjects i cated, let me first give you a very short general accour the brain of man.

This organ, filling, along with its covering membranes blood-vessels, completely the cranial cavity of the head, sists of a large mass of soft and extremely delicate ani tissue, which, from its being of a similar nature with t composing the nerves, is named the nervous tissue texture.

The size of the human brain is subject to great va tion. By the comparison of a very large number of bra it has been found that the average weight of the br in a grown man of this country is about 3 lbs. avoirdup or more precisely 49½ oz.; its bulk is from 80 to 85 cu

nches; and the size does not differ widely from this among he principal European nations.

Much the larger portion of this mass forms the greater rain or cerebrum of anatomists, which you will remark, rom the model I show you, is so large, when viewed from bove, as completely to cover all the other parts. Below the inder part of this comes the cerebellum or smaller brain, rming little more than a tenth of the whole, while below is and towards the middle are certain parts of smaller imensions which connect together the cerebrum and cereellum, and unite both of them to the spinal marrow. These rts are named the cerebral peduncles or pillars, the bridge Varole, and the oblong medulla. The last-named part, or edulla oblongata, passes insensibly at the junction of the ad and neck into the spinal marrow, a long somewhat lindrical column, which is in its structure of the same neral nature as the brain.

From both of these parts (as will be seen from the drawgs exhibited) there arises a series of nerves in pairs, which ss to be distributed by subdivision, or rather separation, their fibres, in almost all the organs of the body; and thus appears that by their anatomical or structural relation, as ll as by their functional association afterwards to be verted to, the brain and spinal marrow constitute together great centre of the nervous system, or cerebro-spinal rvous centre.

The arrangement now mentioned belongs to man in common th all vertebrated animals, and may be regarded in some asure as the basis of the form of the skeleton in vertetes, and of the morphological character which pervades organization of all the classes in that division of the mal kingdom.

n this organization you will observe that two remarkable tures present themselves: 1st, the successive repetition of ts essentially similar in a linear series along and round ntral axis; and 2nd, the precise similarity of the parts on h side (that is right and left) of a middle line or plane, le there is not the same correspondence between those situl before and behind. Longitudinal serial repetition and teral symmetry are therefore anatomical characteristics

of the main parts of the nervous system in vertebrate animals, and you will see from the figures with what remarkable regularity these characters are maintained both in the central masses and in the distributed nerves.

Of the regular nerves now referred to, there are twelve pairs connected with the brain, and thirty-one pairs with the spinal marrow.

By physiological investigation, it is known that the properties of the fibres within the nerves are of two different kinds, viz., sensory and motory, or such that when a nerve is stimulated certain fibres transmit changes in one direction, say from without inwards, while other fibres are connected with the manifestation of vital phenomena in the opposite direction, or from within outwards.

In all the spinal nerves, and in a few of those proceeding from the brain, the fibres of both endowments are enclosed in the same nervous trunk and branches, but in other cerebral nerves all the fibres within one nerve are of the same kind of endowment, that is, either entirely sensory or entirely motory. But in the case of all the spinal nerves, these two sets of fibres, though mixed together in the main nerves, are completely separated at their roots a short distance before these enter the spinal marrow, and thus we have the opportunity, by operating upon the roots, of distinguishing their different properties. We owe to Sir Charles Bell the first experiment which clearly pointed out a difference of function between them, now well known as the afferent or sensory function of the hinder roots, and the efferent or motory function of the anterior roots—a fact which forms the basis of all modern researches concerning the nervous functions.

These nerve roots, both cerebral and spinal, penetrate separately for a certain depth into the central organs, and there pass into some intimate organic union and communication with their substance, and this we name the real or deep origin of the several nerves.

The peripheral nerves are composed of very numerous fibres bound together in a common sheath, but yet so arranged that each fibre runs its course from end to end almost without division, and entirely without union or direct communication with those lying beside it.

Although nervous and electrical action are probably very different, yet the individual fibres of the nerves may not be inaptly compared to the copper conducting wires of an electric or galvanic battery, and especially to such as are insulated by a covering of gutta percha, for it appears that a certain finer inner filament, or a bundle of such fine filaments, is the more immediate seat of nervous action or transmission of nervous influence, while these filaments are surrounded individually or in bundles by materials of a fatty and membraneous nature, which take no part in the nervous action.

In the brain and spinal marrow on the other hand, as also in the so-called ganglia or nerve-knots, which are in some measure central nerve organs, the nervous texture occurs in two very distinct forms. One of these is of a whitish and fibrous nature, and indeed very similar to that just now described as belonging to the peripheral nerves; the other is composed of minute corpuscles of peculiar structure, similar to the microscopic bodies now recognised by anatomists as organised cells; and it is probable that these two kinds of nerve texture are everywhere in direct union or communication with each other in the central organs of the nervous system.

The fibrous component of the central organs is generally held to perform the same office in them as in the peripheral nerves, that is, one of transmission of nervous action from one part to another; but the corpuscular or cellular elements have probably other functions which may be considered to belong specially to central organs, those namely of establishing communication between nerves of different endowments, of transferring in this way nervous action from one nerve to another, and even it may be of originating influences which excite or otherwise act upon the nerves which are in connection with them.

From this it appears that the corpuscular or cellular nervous element is to be looked upon as the peculiarly central one, even although the fibrous transmitting or internuncial nerve substance also enters into the composition of a central organ.

With reference to these two components of the nervous substance in the brain and spinal marrow, it only farther

remains to be stated that, though often much mixed up together, they are generally to be distinguished by a difference of colour, the part in which the fibrous structure prevails being of a brilliant creamy whitish colour, and receiving the name of medullary or white, that in which the cells abound being of a reddish grey colour, and usually called the grey or cineritious matter. In the brain of man and the higher animals, there are three situations in which the grey or corpuscular elements are accumulated in greatest quantity, viz.: 1st, at the places of origin of the roots of the several cerebral nerves; 2nd, in certain large and distinct masses placed in the deeper region or near the base of the brain, forming what have been called the basal cerebral ganglia, of which the corpora striata, the thalami optici, and the corpora quadrigemina are the principal; and 3rd, in a layer occupying the surface of the cerebrum and cerebellum, where from its being thrown into a plaited or frilled form, in what are termed the convolutions of the cerebrum and the laminæ of the cerebellum, a much larger quantity of the grey substance exists than if it had been a simple or plane bounding layer.

I need scarcely say that the brain of man, as I have now briefly described it, is a very different organ from that of a fish or a reptile, or even from that of birds and many quadrupeds, as a glance at the series of drawings representing the brains of some of these animals will at once convince you. But in now proceeding with the discussion of the topics which are to form the special subjects of my lecture, it will be my object to show you that, notwithstanding the wide differences which are apparent in the form and structure of this organ in man and the various vertebrated animals, there undoubtedly exists a general plan of remarkable uniformity upon which it is constructed in them all.

I. We shall first consider the process by which the brain originates in the embryo, and the changes of evolution or development by which it attains to its perfect condition.

Like all the other organs of the body, the brain takes its origin in the germinal part of the egg by a gradual process

of transformation of the organised elementary particles or cells composing the germ, and is gradually built up and moulded into shape by the multiplication of these formative cells, their individual differentiation and their arrangement or grouping in new combinations. There is nothing at first, then, in the form or structure of the egg or its germ, which has any semblance of the future brain or other organs. They are entirely of new formation, and the process by which they come into first existence, and gradually attain their complete form and structure, is not by any means, as was once supposed, one of mere unfolding and coming into sight of parts previously existing, though too small or obscure to be perceptible, but one of entirely new combinations of the organic elements. The brain therefore presents at its origin a totally different form from that which belongs to its complete state, and goes through a number of transformations, both morphological and textural, in the passage from its simplest primitive condition to the adult form and structure.

It may not be out of place to dwell here for a short time on the nature and source of the formative substance from which the primitive parts of the embryo take their origin. The formative part of the egg, which is the source of all new production, consists of a flat expansion or layer of protoplasmic cells, called the blastoderm or germinal membrane, and this membrane has itself arisen by a very remarkable process of self-division and consequent multiplication occurring in the protoplasmic cells, of which it is made up; so that, if we trace back the steps of this process, we shall be conducted at last to a single germinal cell, from which the numerous progeny of blastodermic cells has been derived.

It is now known that this descent of the formative cells of the blastoderm from the original germ-cell, by a process of cleavage or multiplication, is a universal phenomenon in the first production of animals; and as all the organs of the new animal are formed from the blastodermic cells, it follows that they are all derived from the original single microscopic germ-cell. The brain, then, with all its complicated construction and delicate and intricate minute texture, and all its wonderful functional activity in connection with feeling,

action, and thought, has originally sprung from a part of cell-progeny of the original germ-cell of the egg, and all qualities, powers, and properties, which this organ posses by hereditary transmission, must have entered it by me of the extremely minute particle constituting the first ge cell of the egg.

I can believe that some will be disposed to ask here, h is it possible to observe such things? for I have stated o that which is matter of observation; and as I cannot stop explain everything, I will only say in a word that it mainly by the investigation of the changes during incubat of the egg of the common fowl or other birds, that observation of the origin of the first traces of embryo formation has been made, but that supplemental obser tions, both in quadrupeds and in many of the lower anim leave no doubt whatever that the main steps in the pro are similar throughout all the classes of the vertebrate d sion of animals, and that there is even sufficient evide before us to prove that the process is essentially the same the human species.

And now, returning to the subject with which we more immediately occupied—as I must not attempt to desc the whole process of cerebral development—I will endeav to select some of the more prominent facts, and will trus your gathering more of the details from an inspection of illustrations which are before you.

In the first place, when the embryo chick, or rather thickened plate of formative tissue of the blastoderm wh represents its earliest trace, is about a tenth of an inch length, and in the course of the first day of incubation, th is gradually formed, along with a median longitudinal thi ening of the plate, a median linear depression or gro running through the greater part of the plate. The walls of this groove now thicken more and more, and t rise upwards from the plane surface; and, somewhat la the two ridges thus formed coming to arch over the gro approach each other and finally coalesce at their marg first towards the middle and later all along, so as to enc a canal, constituting the walls of the simple cerebro-sp cavity, and the rudiments of the brain and spinal mar

contained within them; and we very soon perceive a widening of this canal at the end which is afterwards found to be occupied by the brain.

Now it is important to note that it is solely the uppermost of the layers of the blastoderm which is at first concerned in thus laying the foundation of the great nervous centres; and further, that as the rest of this upper layer, extending outwards from the seat of the primitive groove and canal now described, is entirely devoted to the formation of the epidermis or cuticular covering of the embryo, there is actual original continuity of formative tissue between the cuticle and the great nervous centre. In fact, we can see the commencement of thickening, by cell multiplication, in the deeper part of the layer, preparatory to the formation of the cerebral and spinal rudiments, before the canal containing them is shut in, and this deeper layer has therefore been distinguished from the superficial or corneous by the name of nervous layer.

Simultaneously with the deposit and enclosure of the first rudiments of the great nervous centres, another important phenomenon of vertebral development demands our attention, viz., the appearance along the sides of the cerebro-spinal canal, and in the thickness of the ridges which have enclosed it, of a series of transverse strongly marked divisions, beginning with only one or two, in what corresponds to the region of the neck, but speedily increasing in number in a direction backwards or towards the trunk and tail end of the embryo. This cross cleavage of the primitive embryo, which does not at first affect the cephalic portion, but belongs only to the trunk of the body, is the indication or the precursor of that serial division or metameral repetition of parts, which is ever afterwards characteristic of the vertebrated form of the animal. It is not, however, exactly a division into the future vertebral pieces of the skeleton, but rather the forecast of a transverse division into segments, each of which comprises the elements of formation of the pairs of spinal nerves, the bones and muscles, and other parts belonging to each zone of the body. These divisions have therefore the morphological nature of segments of the trunk of the body.

A third phenomenon may also be stated as closely related

to the formation of the first rudiments of the vertebrate embryo, in the appearance of a median cellular column below the greater part of the cerebro-spinal canal, and in the place afterwards occupied by the bodies of the vertebrae. This is the chorda dorsalis or notochord, an elementary and probably only a transitory cellular structure, round which the cartilaginous and bony substance of the vertebral bodies is afterwards deposited, and of some significance in the history of morphology and development, from its constancy and its intimate relation to the rest of the vertebrate structure.

The embryonic rudiment therefore presents, from its earliest appearance, features which are manifestly characteristic of the vertebrate organization; and these are, as it were, moulded round the great nervous centre, having as its earliest form that of an elongated tube of formative tissue dilated at its cephalic or cerebral end, and cylindrical throughout its spinal or trunk portion. This neural axis or centre arises by involution of a part of the upper or outer layer of the blastoderm, and is placed immediately above that primitive axial column, the notochord, which occupies the centre of the future spinal column of the skeleton.

Confining our attention now to the cephalic or cerebral portion of the nervous centre, one of the first important changes which occurs in its early development consists in an alteration of its simple pyriform shape into that of three vesicular dilatations, with intervening constrictions in the wall of the primitive brain. These three dilatations have been named the first, second, and third primary encephalic vesicles, and we shall now advert to the manner in which they give rise to the typical constituent parts of the brain.

Of the three vesicular dilations now mentioned, the second or middle one remains comparatively unchanged, and may even be traced to a persistent part of the adult brain. It forms the mid-brain or mesencephalon, and corresponds most nearly with that part of the adult brain which receives the name of the fourfold bodies, or corpora quadrigemina.

A part of the first and third vesicles may also be traced as persistent, the first in what is called the optic beds or thalami, and the third in the medulla oblongata which joins the spinal marrow; but both are greatly modified by the

development of other parts in connection with them. For, in the first place, on either side of the anterior primary cerebral vesicle, there takes place a wide projection or dilatation of its wall, accompanied by a corresponding modification of the outer wall of the head, which is connected with the formation of the first rudiment of the nervous part of the eyes or retina; and, in the next place, there are formed, by extension forwards from the same anterior primary vesicle, two additional vesicles which become afterwards the cerebral hemispheres.

In connection with the third primary vesicle again, the first rudiment of the cerebellum makes its appearance by an arched thickening of the medullary wall, which stretches across the upper part of the vesicle immediately behind the middle one.

Thus the tripartite form of the primary brain is rapidly modified into one in which five parts are distinguishable in a series from before backwards, and as these parts exist in the brains of all vertebrate animals, and constitute the principal basis of formation of the permanent brain structures, they are regarded as typical, and may be enumerated under the following designations, viz.:—

1. The anterior vesicles becoming double at an early period, developed by extension from the first primary vesicle, and named *Fore-brain* or *Prosencephalon.*

2. The part of the first primary vesicle which remains as optic thalami, and which may be called the *Inter-brain* or *Diencephalon*, or the *Thalamencephalon.*

3. The second primary vesicle holding a middle place, and therefore named *Mid-brain* or *Mesencephalon.*

4. The cerebellum developed behind in connection with the third vesicle, and thus receiving the name of *Hind-brain* or *Epencephalon.*

5. The remaining part of the third primary vesicle which persists as medulla oblongata, and being lowest of all, is called *After-brain* or *Metencephalon.*

Had our time permitted it, I would gladly have explained to you in detail the steps by which, out of these five fundamental divisions, the various parts of the brains of different vertebral animals come to be formed; but as this cannot be

done, I must content myself with a reference to some of the more important of the phenomena, keeping strictly in view the relations of the five fundamental parts already mentioned, but adverting more particularly to the cerebrum and cerebellum which attain very large proportions in man and the higher animals. In doing so it will be expedient, for the sake of greater simplicity, to state the developmental changes under the several heads of the modification, in form, size, and position in each of the parts, caused, 1st by the accumulation in certain parts of large quantities of new substance; 2nd, by the contraction, expansion, or other changes of the internal cavities; and 3rd, by textural alteration or differentiation. The two first series of changes may, in a great measure, be considered together; and it may be well, therefore, to say a few words here of the nature of the internal cavities of the brain, or so-called ventricles which have not been previously mentioned.

It has already been stated that, in the first form presented by the cerebro-spinal centre, there is a general internal cavity enclosed by the wall of primitive nerve substance of which it is formed, and we shall find that the permanent ventricles or internal cavities of the brain are only the persistent, though greatly altered, conditions of certain parts of the original simple tubular cavity.

Within the whole of the spinal part of the cerebro-spinal nervous centre, the canal comes to be reduced in the progress of development to almost microscopic size by the gradual encroachment of the solid substance of the spinal marrow which completely surrounds it; but in the cephalic portion of the primitive nervous tube much greater variety of changes occurs. Thus, to begin with the part next the spinal marrow in the region of the medulla oblongata and cerebellum, a transverse widening of the internal cavity in a flattened rhomboidal form, gives rise to what is called the fourth ventricle. In the mid-brain, again, the size of the cavity is suddenly reduced by the increased solidity of the surrounding substance to the condition of a narrow passage, which is prolonged forwards till it reaches the region of the thalami optici, thus forming the aqueduct of Sylvius of anatomists. Within the thalamencephalon the internal cavity is again

dilated and deepened, and forms the third ventricle in the site of the first primary vesicle, which was originally the foremost part of the embryonic brain. But the two cerebral hemispheres, it has already been stated, are developed by an expansion forward of the wall of this first primary ventricle; and as this expansion becomes double, and each hemisphere comes to be occupied by a prolongation or extension of the general ventricular cavity of the primitive brain, the two cavities thus formed constitute the so-called lateral ventricles of the brain, and their common communication with the third ventricle is the aperture called foramen of Monro. This is an interesting fact in connection with the morbid dilatation of the ventricular cavities which occurs in the disease of water in the head or hydrocephalus, which sometimes takes place to such an extent as to expand the whole brain into an enormous bladder, with comparatively thin walls of brain substance.

From what I have previously said, it must have been seen that the substance forming the wall of the primitive vesicular brain is nearly of equal thickness throughout; but in the course of development there soon takes place a great change in this respect. Some parts of the wall, especially on its upper side, becoming relatively thinner, so that the nervous substance is even entirely lost, or, as it were, worn through, and the internal cavity opened up as far as the covering membranes; while other parts of the wall, especially in the lower and lateral regions, undergo a great proportional thickening by the growth of their material, and thus not only are certain of the fundamental vesicles expanded in their dimensions, but new parts are also added in connection with them.

It is also necessary to advert here to another kind of change which accompanies, and we might almost say determines, the mode of development of the head and brain—I mean the adhesion of the brain at certain places to the interior of the cranial cavity, which, when elongation of the whole and different relative expansion of some of the parts take place, is the cause of peculiar inflections both of the brain itself and of the cranial wall.

The most remarkable change of this kind occurs at a

place which is immediately below what is originally the foremost part of the primitive brain, in a narrow prolongation downwards of the first primary vesicle, termed the infundibulum, where the brain is, as it were, pinned down to that part of the cranium which is afterwards occupied by the pituitary gland in the so-called "Turkish saddle." Now, the infundibulum of the brain is in all animals united with the pituitary gland at this place, and the union so effected seems to exercise some important influence on the whole form and development of the head; for while the expanding hemispheres of the brain become subsequently projected forward with the forehead, and upward and backward with the vault of the cranium, the original first vesicle or thalamencephalon remains adherent to the base of the skull, and thus is caused a bending of the whole skull round this point, which determines much of the later position and direction of the surrounding parts both of the cranium and face.

In endeavouring to trace further, in the briefest possible manner, the successive steps by which the fundamental parts of the brain are developed into those of the fully-formed organ, let us begin from behind, and take first those which preserve most nearly their earlier or embryonic form and relations.

1. In the part of the fifth fundamental vesicle which becomes the medulla oblongata, we observe at a comparatively early period of fœtal life a great difference in the growth of nervous substance in the upper and lower part; and in the former, as already noticed in connection with the fourth ventricle, so complete an atrophy, or want of new deposit, that the wall gradually thins away and disappears as far as the covering membranes, so as to open up the cavity of the fourth ventricle in that direction.

In the lower and lateral parts, on the contrary, a rapid and great increase of the solid nervous substance takes place, which leads to the formation of the constituent strands of white substance with their accompanying contained nodules of grey matter, such as the anterior pyramids, the olivary, and the restiform bodies, with the posterior pyramids.

2. The vesicle, which we have termed mid-brain in the

mbryo, retains for a considerable time the most prominent lace among the five fundamental encephalic divisions, and rom its superior size, as well as the upward bending of the ead, gives rise to a strongly marked projection of the part f the head which it occupies. It is at first single, and is ccupied internally by a portion of the general ventricular avity; but in the course of the middle third of the period f development it undergoes a great change of relation by the uperior relative development of other parts, and is itself reatly altered by internal processes of growth. Thus it ecomes divided first into two by a median depression on its pper surface—constituting the corpora bigemina of a certain etal stage and of animals up to the class of birds inclu-ive—and by a subsequent transverse groove is again sub-ivided, so as to show superiorly the fourfold character which has given to these bodies, in man and quadrupeds, the ame of corpora quadrigemina.

By the great increase of deposit of the nerve substance, oth white and grey, in these bodies and below them, the entricular cavity (as previously stated) is gradually more nd more contracted till it is reduced to the narrow tubular orm of the aqueduct of Sylvius.

In the base of this fundamental part of the brain, here are found on either side the large masses of white or brous nerve substance, termed crura cerebri or cerebral eduncles, which form a principal means of union between he medulla oblongata below and the cerebral hemispheres bove.

3. In connection with the thalamencephalon, or that part f the primitive brain which was originally placed farthest orward, the cerebral hemispheres arise at an early period by n extension forward and upward of the wall of the first rimary encephalic vesicle, in a manner afterwards to be pecially referred to. At present we have to do only with he lower and back part of this vesicle which remains as the nter-brain or thalamencephalon, in which several important hanges deserve our attention.

a. First of all, the largely increased deposit of solid ervous substance in the lower and lateral parts of this ivision of the primitive brain, on each side of the ventricu-

lar cavity, forms the foundation of one pair of the great cerebral ganglia—viz., the thalami optici, with which the original pedicles of the optic nerves are connected, and these are separated by the cavity of the third ventricle, except at the place where they are brought into union by the grey commissure.

b. The prolongation downwards of the thin cerebral wall, with the cavity of the third ventricle within, in the shape of a funnel, the infundibulum, and the union of this, already referred to, with the pituitary body or hypophysis cerebri; and

c. The opening up, so far as the nervous wall is concerned, of the ventricular cavity in its upper and anterior part by a thinning process, nearly of the same nature as that previously mentioned for the fourth ventricle, and the development in the hinder part of the upper wall of that remarkable appendage to this part of the brain, the pineal gland, the Epiphysis cerebri, held by Descartes, on purely hypothetical grounds, to be the seat of the thinking principle. The remarkable constancy of the two bodies now mentioned (pituitary body and pineal gland), and the uniformity in their respective structure and mode of origin in all animals, from man down to fishes, constitute a set of important anatomical facts which are of doubtful interpretation, and require further elucidation from the history of development.

4. The cerebellum, or hind brain, which forms the fourth division of the primitive brain, as enumerated from the front, begins in the forepart of the third primary encephalic vesicle, by the growth of a bridge of nervous matter over the remainder of the vesicle, which, as already explained, forms the medulla oblongata and fourth ventricle. The cerebellum is at first a single and median lappet, but, its lateral parts afterwards increasing, it comes to consist, in more advanced stages of development, of median and lateral lobes. As these last increase a lower bridge of nervous matter crosses the base of the brain between them, and unites them by a sort of commissure. This is the pons Varolii, or mesocephalon, or nodus cerebri of anatomists.

The later development of the cerebellum is attended with the formation of deep furrows in its surface, dividing it into

plates, and the distinction of the white substance within, and the peculiar grey or cellular layer externally, the disposition of which over the subdivided laminæ of the white gives, in a vertical section, that remarkable appearance which has received the name of arbor vitæ.

5. Lastly, the cerebral hemispheres appear to be double from a very early period; but they are probably not so from the very first, for they owe their origin to the extension of a single median part of the wall of the first primary encephalic vesicle together with its ventricular cavity. In fact, the fore and upper part of this vesicle becomes separated from the rest by a cleft or fissure, which, proceeding from behind forwards at each side, cuts off, as it were, the strictly cerebral part in front and above from the thalamencephalal part which remains below and behind.

The extension of the ventricular cavity takes place at what afterwards becomes the foramen of Monro, and is at first only single and median; but as the mass of the cerebral hemispheres rapidly expands outwards and upwards, and comes to be divided into two lateral masses by a partition passing from before backwards, the right and left hemisphere vesicles become distinct, and the ventricular cavity, expanding along with each, comes to form the right and left lateral ventricles, communicating with the third or median ventricle by the single and now reduced foramen of Monro.

In connection with the development of this, which becomes much the largest part of the human brain, the following circumstances are to be noted:—1st, The formation of the anterior and largest basal encephalic ganglia, composed mainly of grey substance, viz., the corpora striata, by a rapid growth of the solid nervous substance in the lower and lateral parts of the commencing hemispheres. These bodies lie chiefly below the expanding lateral ventricles, but to the sides and above, like the thalami, they are in connection with the substance of the cerebral hemispheres by a radiated or fan-shaped expansion of the white nervous tissue, termed corona radiata. These ganglia, like the thalami, are united below, through the crura cerebri, with the hinder parts of the brain. 2nd, A rapid increase of the upper and superficial part of the two hemispheres now takes place, by which

their relation to the other parts of the brain comes soon to be greatly changed; for by growing forward they project more and more beyond the region of the first primary vesicle, which, it will be remembered, never advances farther forward than the pituitary fossa (lamina terminalis); while, in expanding upwards, they take the place previously occupied by the mid-brain, and fill the most prominent part of the head; and by a downward and lateral enlargement they form the temporal lobes. Thus frontal, parietal, and temporal lobes come to be distinguishable, and somewhat later, by a farther increase posteriorly, the hindmost parts of the cerebral hemispheres constituting the occipital lobes are formed, and the cerebrum at last covers over completely all the lower parts of the brain. Thus the cerebral hemispheres, which, in the early embryo of all vertebrates temporarily, and in adult fishes permanently, are comparatively small and placed foremost in the linear series of vesicles constituting the fundamental brain, come in man and higher animals to surpass all the other parts in magnitude, and not only to project far before and above them all, but to cover them in completely, so as to hide them from above.

Two sets of connecting bands, between remote parts of the cerebral hemispheres, require to be noticed: one of these, running from before backwards, connects the lower and anterior parts of the cerebrum with the hinder and lateral parts, in the shape of two pillars of white nervous substance, termed the fornix. The other is transverse between the adjacent parts of the cerebral hemispheres, and forms what is called the great commissure, or corpus callosum. This consists of white or fibrous substance in a large mass, which begins to be formed in front, and as the hemispheres grow gradually, extends backwards, so as to unite their inner surfaces, to the extent in the adult of between three and four inches. The fibres of this commissure, which cover in the lateral ventricles, spread themselves in each hemisphere to every part of their wide surface.

Lastly, the expansion of the cerebral hemispheres is attended with the increase of the superficial layer of grey substance, and along with that the division of the upper or expanded part, termed the mantle, into lobes and convolutions.

The upper wall of the vesicular hemispheres is at first quite smooth. The first appearance of division into lobes is that of a blunt notch between the frontal and temporal parts below, in what afterwards becomes the Sylvian fissure. In the fourth and fifth months there appear the vertical fissure separating the parietal and occipital lobes, and the transverse fissure named after Rolando, which divides the frontal and parietal lobes superiorly, and which is peculiarly characteristic of the cerebral type of man and the Simiæ. Another of the earliest furrows appearing in the fœtal brain, is that termed hippocampal, corresponding, on the internal surface of the hemisphere, with the peculiar projection of substance in the ventricle termed hippocampus major.

Thus the great lobes of the cerebral hemispheres, named frontal, parietal, occipital, temporal, and central (island of Reil), are marked off from one another, and by the successive formation of other grooves and the corresponding growth of the cerebral substance between, there gradually follows the formation of the various convolutions, which attain their highest development in the adult human brain; such as the great convolution over the corpus callosum, that surrounding the fissure of Sylvius, the ascending frontal and ascending parietal convolutions, the three series of frontal, occipital, and temporal convolutions, the upper and lower series of parietal convolutions, the annectant, and the various subordinate ones, which come all to be more or less distinctly indicated in the brain by the time of birth. But still they are not fully formed even then, for considerable deepening of the grooves between the convolutions, and further subdivision into subordinate ones, occurs to some extent in the first years of infancy.

We know too little of the progress of minute textural development of the brain substance to enable us to make any statement in regard to this subject which would be of interest. We only know that the brain, like all the other growing parts, is originally formed entirely of cells, that it is by differential transformation of these that the fibrous white substance, as well as the grey cellular constituents of the brain, are produced. Large deposits of grey substance take place in the cerebral ganglia and crura cerebri. The

grey substance on the surface is probably the last formed, as we see it increasing considerably in thickness in the last months of fœtal life; and it is probable that the process of textural development continues to go on in the brain, and more especially in its superficial layer, for years after birth. The investigation of the relation between the more complete textural development of the brain and the progress of the intellect, will some day form a most interesting subject of investigation.

The Olfactory Lobes, with which the nerves of smelling are connected, seem at first to come from the anterior and lower part of the cerebral hemispheres, but when the hemispheres have attained a large size, are found to proceed also by a nervous tract from their lower part, in what is called the uncinate convolution. Thus, it appears that the three principal organs of sense are severally connected with three different parts of the fundamental brain, viz.:—1st, The olfactory with the cerebral lobes; 2nd, the optic with the thalamencephalon, and, later, also with the mid-brain; and, 3rd, the auditory with the medulla oblongata.

II. In now proceeding with the second part of my subject—viz., the evolution of the brain in the animal series, it will not be necessary to occupy so long a time as has been spent upon the first, seeing that the differences of its form and structure observed in the various classes and orders of vertebrate animals may be referred to a graduated series of increasing complication proceeding out of the same fundamental type; so that they may be looked upon, in some measure, as a repetition of those which are presented by the embryonic brain of the higher animals in the successive stages of their development.

Thus, in the whole Class of Fishes the brain retains throughout life more or less of the elementary form which is common to the embryo of all vertebrates; that is, it consists of a series of vesicular enlargements of the medullary tube, single or in pairs, extending forwards from the spinal marrow into the cranial cavity.

In the Cyclostomata, such as the Lampreys, the form approaches most nearly to that of the embryo at the period

en the five fundamental parts previously referred to ae to be distinguished. In such a brain we especially nark the small size of the parts corresponding to the ebrum and cerebellum, the former being sometimes conerably less than the mid-brain, and the latter being no re than a narrow bridge across the primitive medullary)e.

In the Sharks and Skates there is considerable difference the relative size of the parts, the cerebral lobes having ained to larger proportions; but still the five fundamental risions are readily recognised.

In Osseous Fishes some difficulty arises in establishing the mology of the several parts, in consequence of a change in) second and third of the fundamental divisions, which ms to consist in their amalgamation or union, so as to form) large pair of eminences of the brain of these animals, ually named the optic lobes, as well as from the occurrence additional inferior lobes in connection with them.

But in Osseous, still more than in Cartilaginous Fishes, e cerebral hemispheres are still of very inferior size, and e cerebellum has attained no great magnitude. Thus it ppens, that from the small size of the whole brain, and e comparatively large growth of the head, the brain of hes occupies only a small portion of the space within the anial cavity.

In the Amphibia we begin to see a manifest enlargement the cerebral hemispheres in proportion to the other parts s may be seen from the drawing of the frog's brain exhited). But yet, though the cerebrum is now the largest rt of the brain, and thus hides the second division, it aves uncovered the next in order of the series of fundaental parts—viz., the mid-brain, which still makes conlerable projection, while the cerebellum remains of markably small size.

In the class of Reptiles, as in the Turtle (here shown), ie proportional increase of the fore-brain has made further ogress, and the cerebellum now attains a larger size, espeally in its median part; and in the highest members of iis class, such as the Crocodile, we perceive the commenceent of a transverse grooving of the cerebellum, giving rise

to the foliation or laminar division, which is carried much farther in birds and mammals.

The vesicles of the mid-brain, retaining a considerable size, and with a ventricular hollow within each, still remain exposed behind the cerebrum; but the inter-brain is sunk down in the deep cleft between the fore-brain and mid-brain, and is surmounted by the pineal gland.

In Birds, although the vesicles of the mid-brain are still of considerable size, yet, from the great enlargement of the cerebral hemispheres, they now are partially hidden; and from the increase of solid substance within them, their internal cavities or ventricles begin to contract. The cerebral hemispheres now fill the large and expanded vault of the cranium, and a partial division by a blunt notch, indicates the division of the cerebrum into frontal and temporo-parietal parts. The middle part of the cerebellum has also made rapid strides, and is distinctly laminated transversely, and the difference of its internal white and external grey substance becomes apparent. At the sides, too, of the cerebellum additional parts make their appearance in the flocculi, which may be looked upon as the earliest form of the lateral lobes.

As yet there is no pons Varolii, nor any corpus callosum as the great commissure of the cerebrum. The lateral ventricles extend fully within each cerebral hemisphere, and communicate with the third ventricle by the foramen of Monro. In the floor of these cavities are now seen the corpora striata and thalami optici, but there are no posterior lobes of the cerebrum; and it is doubtful if any parts corresponding to the fornix, or hippocampus major, exist. The two corpora striata are united by means of an anterior commissure.

In Mammalia, together with a general enlargement of the cerebral hemispheres, we find now existing a transverse commissure, or corpus callosum, uniting them together, of small size, and confined to the fore-part of the hemispheres in the lower orders—such as the Monotremes and the Marsupiate animals, as well as some Edentata—and gradually increasing in size and extending further and further back in the higher orders as the cerebral hemispheres become more fully developed.

In the lower orders of Mammals the hemispheres are comparatively small and simple, and do not present any division into convolutions, and very little distinction even of the lobes, which are recognised in the higher orders. It is only in this class, therefore, that the cerebral hemispheres, attaining their full development, gradually progress, as it were, backwards, and cover successively the mid-brain, cerebellum, and medulla oblongata, as is found at last in the higher Simiae and in Man. The general expansion of the cerebral hemispheres is attended by a great enlargement of the frontal, parietal, and occipital bones, as well as in part the squamous portion of the temporal bone enclosing the cranial cavity. The development of a posterior lobe only takes place in the higher orders, more especially the Simiae; and in them, too, the proportional enlargement of the frontal lobes brings that part of the cerebrum more and more forward over the nasal cavities and the olfactory bulbs, so that, while these bulbs were at first placed in front of the cerebrum, they come, by relative diminution of size and change of position, to be thrown quite below the frontal part of the hemispheres.

The lateral parts of the cerebellum also undergo a gradual increase in this class; and along with this appears, in gradually increasing size, the pons Varolii, which constitutes, in some measure, a transverse commissure between the opposite sides of the cerebellum.

Along with the corpus callosum or great transverse commissure of the cerebrum, another part makes its appearance in the mammalian brain, and is developed in proportion to its advance in organization. This is the part termed fornix, which lies immediately below the corpus callosum, and constitutes a pair of longitudinal bands, stretching from the hippocampus major and minor of the temporal and occipital lobes on each side forwards in two pillars, which descend into the base of the brain behind the infundibulum into the white projection termed corpus mammillare, or when double in the higher Mammalia, the corpora mammillaria.

The growth of the fornix is accompanied by the formation of the septum lucidum which intervenes between it and the corpus callosum, forms a median separation between the two

lateral ventricles, and contains between its two layers the cavity termed the fifth ventricle.

The mid-brain retains the most simple bifid form of the corpora bigemina in the Monotremes and Didelphia, but in all higher animals these bodies are separated by a transverse groove into four, the corpora quadrigemina, of which the anterior pair is usually the largest.

The corpora striata and thalami optici become more fully developed as the cerebral ganglia of Mammalia. The first are united transversely by the anterior commissure, the size of which is generally in inverse proportion to that of the corpus callosum. The thalami are united by the small posterior commissure, and also by the grey commissure, the latter of which, however, is not constant.

With respect to the convoluted condition of the cerebral hemispheres, to which considerable interest is attached from its supposed closer relation to the higher cerebral functions, it appears that, while it is no doubt true that in the main the convolutions become most numerous and deepest in animals belonging to the higher orders, and reach their highest degree of complication in Man and the Anthropoid Apes, there is not any regular gradation of increase to be observed in passing through the whole series of Mammalia, and within each order or large group there may be found very great variations in the degree of convolution. Thus even in the lowest, such as the Monotremes, the greatest difference is observable between the nearly smooth surfaced brain of the Ornithorynchus, and the comparatively highly convoluted brain of the Echidna; and the same may be affirmed of the Marsupial animals; while in the order of Primates it is found that some, as the Marmozet and other Hapalidae, present cerebral hemispheres almost entirely destitute of convolutions, which differ greatly from those of other monkeys, and contrast strongly with the highly developed convolutions of man and the anthropomorphous apes.

Among the other orders of Mammalia, the Edentata, Rodentia, and Insectivora are distinguished by the absence or small size of the convolutions; while in Carnivora, Ruminantia, Pachydermata, and Cetacea, they are moderately or more fully developed.

Anatomists have not yet succeeded in reducing the cerebral convolutions of the different Mammalia to a common type, and it would rather appear that various types or forms of convolutions prevail in different orders or larger groups, as is well illustrated by the selection of drawings of the upper and lateral surfaces of the series of the brains of animals placed before you. Without attempting to trace these differences further, I will here confine my remarks to that type of the convolutions which appears to pervade the whole group of the primates, and which, gradually increasing in complexity, leads to the higher form observed in the human brain.

In its most developed form this plan or type may be briefly described as follows.

Each cerebral hemisphere presents a division into five lobes, which are named the frontal, parietal, temporo-sphenoidal, occipital, and central. In each of these lobes there is a superficial division into convolutions, some of which are principal and others subordinate. In each of the frontal, temporo-sphenoidal, and occipital lobes, we can distinguish three series of convolutions, which are named from their position from above downwards, superior, middle, and inferior; in the central lobe, or island of Reil, there are five or six superficial radial convolutions, and in the parietal lobe a subdivision by the inter-parietal fissure separates the upper and lower parietal lobules, which again are subdivided into convolutions.

Besides these, the following principal convolutions are distinguished, viz.: the ascending frontal and the ascending parietal, situated the one before and the other behind the fissure of Rolando; the callosal and hippocampal convolutions which surround the corpus callosum; and the angular and supramarginal convolutions at the back of the fissure of Sylvius.

To these may be added, as distinguishing peculiarities in the convoluted form of Man and the higher Apes, the cuneus or wedge on the inside of the occipital lobe, and the precuneus or quadrilateral space on the inside of the parietal lobe; and as developed more and more as we rise in the scale of the Simiae, the annectant convolutions which intervene

een the parietal and temporo-sphenoidal lobes on the hand, and the occipital lobes on the other.

'ithout attempting to trace the varieties of these contions throughout the order of Primates, I will only t your attention to the series of drawings which repre- the brains of several apes alongside of those of the ıpansee and of Man, from which you will easily see that, in the lowest, such as the Marmozet, there is an st entire absence of any division of the cerebrum into olutions, yet in others higher in the scale, as soon as olutions exist, we can perceive that they are modelled the same plan as that which attains its highest develop- in man. Thus you will observe that in the brain of Barbary Monkey, familiar to us all, while the fissure of us divides very clearly the frontal and temporal lobes, is also present a marked fissure which separates an ital or posterior lobe from the rest, and that there is transverse fissure of Rolando, before and behind which he ascending frontal and ascending parietal convolu- so peculiarly characteristic of the human brain.

the three views of the Chimpansee's brain, magnified ee times its natural size—and with which I may men- n passing, the brains of the gorilla and orang agree in arkable manner—you will readily see the near approach is made to the human form in the general development convolutions in nearly all the parts, in the complete ng of the cerebellum by the overhanging posterior al lobes, in the double state of the corpora albicantia, ıall size of the olfactory lobes, and in many other cir- ances which I need not attempt to particularise. The marked differential character in the brains of the 'opoid Apes is perhaps in the lower development of the ant convolutions between the occipital parietal and ral lobes, and the small number of convolutions in the al lobe itself.

The gradual increase in the size of the brain, as red with that of the body, and especially of the cere- and cerebellum, which is observed as we rise in the scale, has been generally held to bear some intimate

proportional relation to a corresponding increase of their nervous and mental endowments. But although in the main this view may be well founded, we shall see that it is subject to many exceptions which are still unexplained. I will now, therefore, under the third division of our subject call your attention very shortly to this difference of size, confining my remarks, however, to the brains of the higher animals and of man.

Information as to the size of brains might, of course, be obtained most directly by actual measurement of their dimensions and weight; but, as this is often difficult, we have recourse also to the measurement of the capacity of the cranium, the cavity of which is completely filled by the brain and its accessories in all the higher animals.

I shall be able to illustrate the difference of brain size, as ascertained in the last-mentioned way, by reference to a number of casts, now shown, of the cranial cavity in different animals and races of man, which are a selection from those made for the Museum of the College of Surgeons, London, and which I have placed in the Hunterian Museum of our University; and, in connection with this, I refer you also to the short tabular statement which is placed before you, in which the brain weight and cranial capacity, and their relation to the weight of the body, are compared in man and a few animals (see p. 34).

To begin with my remarks on the size of the human brain, it may be stated as the result of an extended inquiry over several thousand skulls made by different observers, that the cranial capacity is on the whole greater among the highly civilised than among the savage races, and that there is even a very manifest difference to be found among persons belonging to the same race, between the crania of persons of higher mental cultivation and acknowledged ability and those of the uneducated class and of inferior intellectual powers.

The amount of this difference is as yet only imperfectly known, and has sometimes been over-estimated by those who have adopted strong pre-conceived views on the subject; but, making due allowance for the chances of error, it may be stated as probably amounting to from 5 to 7½ per cent. in persons of the same race, and to about double that range in those of different races.

Thus, the average adult brain of men of this country ŀ
taken at 3 lbs., or, more precisely, at 49½ oz. avoir.,* a
average specific gravity of 1040, this would give a bul
82·5 cubic inches of brain substance; and if we make
addition of 10 per cent. for loss by membranes, fluid, &c.,
cranial capacity for such a brain may be calculated at al
90 cubic inches, and conversely we may calculate the l
and weight of the brain from the known cranial capac
If, therefore, the brain of the uneducated class falls 2·5
below the average, while that of the more cultivated pers
rises to the same amount above it, or to 52 oz., we r
estimate these brain sizes as corresponding respectively w
brain bulks of 78 and 87 cubic inches, and with cra
capacities of about 88 and 97 cubic inches.

If, again, we compare the average brains of the Europ
races with those of the lowest savages, such as the A
tralians, we shall find the latter to be of an average wei
of 42 oz., and to correspond with a brain bulk of about
cubic inches, and a cranial capacity of about 78 cubic inch

But there are examples outside the averages in all ra
of very considerable variations in brain-size and cran
capacity, although the range of these extreme variatic
differs like the averages themselves in different races. Th
there have been known brains belonging to persons of t
European races of the weight of 60, or even as high as
oz. avoir., as in the well-known instance of Cuvier the gre
naturalist, and this would correspond to a brain bulk
108 cubic inches, and a cranial capacity of 118 cul
inches; while, on the other hand, it is found, even amo
the European races, that the weight of the brain has fall
as low as 32 oz. (the lowest probably compatible with healtl
action), which would correspond to a brain bulk of 53 cub
inches, and a cranial capacity of about 63 cubic inches.

These very great variations in brain size are in a gre
measure, though not altogether, independent of differences
stature in the whole body; and it must be regarded as a r
markable fact, not yet fully understood, that an organ const
tuted as the brain is should, in different members of the sam

* The average weight of the female brain in this country is abo
44 or 44½ oz.

species, be subject to so great a range of difference as that in some cases it should be double the size that it is in others.

The brains of the Anthropoid Apes, the Gorilla, the Chimpansee, and Orang, all of which have now been well described by competent authors, are far inferior to that of man in their dimensions. Even in the Gorilla, which is most similar in stature to man, the brain does not attain more than a third of the weight of the average human brain, and in the Chimpansee and Orang it does not even reach a fourth; and thus in these animals the proportion of the weight of the brain to that of the whole body may be as one to a hundred, or even lower, while the proportion in man is from 1 to 40 to 1 to 50.

In most other animals the proportion, as a general rule, is still lower, and in some indeed very much inferior to that before stated. But here it is proper to state a remarkable fact which has been derived from a consideration of this proportion throughout a wide series of animals—viz., that in general among the largest animals of any order or group the brain does not reach a size which is by any means proportionate to the greater magnitude of the other organs or of the whole body, so that in the smaller members of the same order or group, a considerably greater proportional size of the brain is observed. This fact obtains even in the order of Primates, with man at its head, as is very well illustrated by the example of some of the smallest monkeys, in which, as I and others have found in the Marmozet, the brain holds the proportion of one-twentieth of the weight of the body, or more than double the proportion existing in man.

As a very striking further illustration of the same fact, I may refer to the Whale and the Porpoise in the order Cetacea. Here, for example, is the cast of the brain of a Greenland Whale, which was 75 feet in length, and whose weight could not, at the lowest computation, have been less than 60 tons, and yet the brain only weighed about 6 lbs., or twice the weight of the human brain, thus giving a proportion of brain to body of 1 to 22,600; while the common Porpoise, or Cetacean of our seas, weighing probably not more than 60 lbs., has a brain, of which I now show you the cast, of the weight of 1 lb. or 16 oz., and consequently giving

the enormously superior proportion of one-sixtieth. I may add that both of these brains, but more especially that of the Porpoise, is very fully convoluted.

Among the Pachydermata, too, examples of the same contrast are to be found. Thus the brain of the Elephant, which weighs three times as much as that of man, and is, in fact, the largest brain known among animals, and is very fully convoluted, is yet, from the very great size of the animal, which we may estimate at three or four tons, in a much smaller proportion to the weight of the body than is that of the Pig or other smaller members of the order.

So, too, in the Dog tribe, we shall find that the smallest races have the largest brains in proportion to the body, as may be seen from the two examples noted in the table, in the smaller of which the proportion is the same as in man, while in the larger it is reduced to at least one-third.

The same general fact has long been known in the class of birds, in the smallest of which, such as the Linnets and Finches, the proportion of the brain to the body frequently rises as high as or above one-twentieth.

It must be confessed that we are as yet wholly unacquainted with the significance of this very remarkable fact; and therefore, although it may be to some extent true that, within certain races of man and certain species of animals, the size of the brain varies in proportion to the mental capacity and activity of the central nervous functions, yet it is obvious that we cannot, in the present state of our knowledge, place implicit reliance on size alone as a criterion of perfection of function in the brain.

It is necessary, also, to keep in mind another general fact with respect to the size of the brain and its proportion to the body, viz., that in the young of man and of animals generally, as the brain grows more rapidly than the rest of the body, or is, as it were, more advanced in its development, it necessarily bears a much larger proportion to the body in the fœtus and in early youth, than it does in the adult. In comparing, for example, the skulls of children with those of adults, and those of a young Orang and a young Chimpansee with others which are fully grown, one cannot fail to perceive the very great difference existing between them in

the proportion of the cranium and face, and this depends not only on the large size which the cranium containing the brain has early attained, but also on the inferiority in the development of the jaws and other parts of the face.

The brain of a male child at birth weighs, on an average, nearly three quarters of a pound, or 12 oz.; and if we assume the average weight of the child to be $7\frac{1}{2}$ lbs., or 120 oz., the proportion will be seen to be a tenth. And so rapidly does the brain continue to grow during the early years of childhood, that already by the age of three years it has attained more than three-fourths of its full size; by the age of seven years it has reached the proportion of nine-tenths, and after this, only by slow and comparatively small gradations, it attains the full size between the age of twenty and twenty-five years.

The same precocity of growth in the brain exists among animals in general, as may be seen by reference to the specimens now shown of the young Gorilla, Orang, and Chimpansee skulls; and it is obvious therefore that, in any comparison of the size of brains from the capacity of skulls or otherwise, account must be taken of the age (as well as the sex) of the animals compared.

The relation of the size of the brain to the defect of idiocy is a subject of considerable interest, but time does not admit of my treating it in detail. It is true that there are many instances of idiots in whom the brain has either been of the full average size, or not more below it than occurs in many sane persons. But this need not create surprise when we consider that the functions of the brain may be disordered, and even abolished, by pathological changes of its intimate structure, and that these are sometimes so minute as to escape very close observation. There are, however, other examples, more especially of congenital idiocy, which are coincident with marked inferiority of size, and (as may be seen from one of the drawings presented) with want of due development of the normal cerebral form. It would appear that in general, throughout the human race, no brain under 30 oz. is sufficient for the exercise of its normal functions.

Some of the more remarkable of the relations referred to in the foregoing pages are illustrated by the annexed table:—

TABLE SHOWING SOME EXAMPLES OF THE COMPARATIVE SIZE OF THE BRAIN AND BODY.

EXAMPLES.	Brain-weight in oz. avoir.	Internal Cranial Bulk in cub. in.	Whole Weight of the Body in lbs.	Proportion of Brain to Body-weight.	
Average European Man, ...	(3 lbs.) 48	85—88	140	1 to	46
Child at Birth, ...	12	22	7½	...	10
Chimpansee, ...	10	19	50	...	80
Marmozet, ...	⅓	½	6 oz.	...	20
Middle-sized Dog, ...	3½	6	36	...	164
Small Dog, ...	2½	4½	7	...	45
Elephant, ...	(9 lbs.) 144	300	(3 tons) 6720	...	750
Pig, ...	6	11	94	...	250
Whale, ...	(6 lbs.) 96	650*	(60 tons) 134,400	...	22,600
Porpoise, ...	16	30	60	...	60

* The large cranial bulk in this instance is connected with the enormous size of the roots of the cranial nerves.

IV. In proceeding now to consider, in the fourth place, the relation subsisting between the minute structure of the brain and the manifestation of its functions, or what may be termed the more immediate mechanism of cerebral action, we have to grapple with much the most difficult part of our subject. To attain to a comprehension of this mechanism, the results of physiological experiment and pathological observation must be combined with full and accurate knowledge of the anatomy; but, unfortunately, the investigation of the minute cerebral structure, notwithstanding the great amount of attention recently bestowed upon it by the most competent workers, has not yet led to very definite results; nor have the anatomical facts observed been found to be in entire accordance with the data hitherto supplied from physiological and pathological sources. I must content myself, therefore, with the briefest possible treatment of this part of our subject.

Referring to what was stated in an earlier part of this lecture, it will be remembered that the brain and spinal marrow, as central nervous organs, consist essentially of two very different forms of nerve tissue, viz., the white or fibrous, and the grey or cellular, the first of these being composed of distinct longitudinal fibres, in many respects similar to those of which the peripheral nerves are formed; the other always containing, along with fine interlaced fibres, peculiar corpuscles or cells, which belong almost exclusively to the central nerve organs, and are in some intimate connection with the nerve fibres.

As might be supposed, the office of these two kinds of nervous substance is very different, and yet their concurrence is necessary to the manifestation of almost all kinds of nervous phenomena in the animal body. It is universally admitted by physiologists that the fibrous element, whether existing in the central organs or in the peripheral nerves, is essentially a transmitting medium, or, in other words, that the change which we call nervous action, induced by a stimulus or excitement in a living nerve, is conveyed or conducted along each individual fibre separately, from one end to the other, so long as it holds an uninterrupted course. We do not know, any more than in the case of a galvanic wire, the

intimate nature of the process by which the conducti effected; and it seems not improbable that nerve fibres like galvanic wires, be capable of transmitting their a in both directions equally: but in ordinary circumst the direction of a nerve current can be recognised only the effect resulting. Thus, in the efferent or motor ne the outward conduction of the excitement is immedi indicated by the muscular contraction which follows; v in the case of the afferent or sensory nerves, the cc inwards is manifested by the less direct succession of s tion, or by the transference of the nervous influence thr central nerve-cells to motor fibres, so as to give rise to r muscular contraction. Nor has any characteristic differ in structure been ascertained to exist between the fi of sensory and motory nerves; it is only known that former are, on an average, somewhat less in diameter t the latter.

The corpuscles or cells of the central grey substance, v which many of the nerve fibres are undoubtedly united i more or less direct manner, have obviously a very differ function from that of mere conduction; but as to the nat of nerve-cell action, we can only form very vague conjectu The structure also of the grey substance is as yet very perfectly known. There are probably various forms of it different parts of the central organs, but in the main they agree in possessing the characteristic cellular constituti the cells being mingled as well as united with a varia quantity of nerve fibres, and with finely granular or mo cular substance, and the whole being set in a matrix delicate reticulated connective tissue, permeated by bloc vessels, from which are derived the nutritive materials f the maintenance of the chemico-vital changes which a associated with the nervous as with all living actions.

In the simplest example of a central nerve action, vi that of the transference or conversion of an afferent into motor impulse, which leads to reflex movement, there can l little doubt that it is by the union established between tl two kinds of nerve fibres concerned, by means of the radia ing processes of the intervening cells, that the transferen is effected; and we have every reason to believe that even i

the very complicated automatic or involuntary movements involved in many of our actions, which may go on independently of consciousness, it is entirely by a similar, though doubtless a more complex arrangement of cellular intervention between nerves of different endowments, that the muscles are set in motion.

We do not know, it is true, whether this process of transference is simply one of conduction or something more; but in the more complex forms of central nerve action, if not even in the most simple, it seems probable that the cells are capable of increasing or modifying nerve force, or possibly of initiating it within themselves, as well as of transmitting and transforming that which has been brought to them from without; and we are still more in the dark in regard to that peculiar form of nerve action which is called inhibition.

It is well known that habit or frequent repetition may have the effect both of rendering certain channels of communication or transference more easy, and certain actions more ready to recur, and possibly of establishing new channels not originally existing; but the nature of the instinctive movements which occur in the first acts of the child after birth, such as the first respiration, sucking, crying, and the like, are sufficient proof of an originally existing nervous mechanism, or rather a series of such mechanisms of wonderful complexity, which provides for the natural occurrence of these movements by the reaction of parts of the nervous system on each other, and altogether independently of the will.

Little as we know these mechanisms, however, beyond the certain fact of their existence, it is far more difficult to comprehend in what manner the relation is established between cerebral structure and the higher psychical phenomena of conscious sensation and perception, ideas in their simple and their abstract form, memory, judgment, and volition. And yet physiology and pathology point out in the most unmistakable manner the immediate dependence of them all on cerebral action as a physiological condition.

If this view be admitted, we shall further be disposed to recognise a more close relation of psychical phenomena to the cells of the grey substance than to the fibres of the white,

and more especially to the peculiarly-formed cells of layer of grey matter which covers the surface of the (brum and its convolutions, as the more immediate se the purely mental processes.

The investigation of the structure of parts so highly dowed must therefore be looked upon as one of the r interesting problems presented by the whole range anatomy, and no doubt it is one which will engage attention of men of science during many years to come, v what result, however hopeful I may be, it is not for m attempt at present to determine.

I will rather endeavour, as briefly as possible, to expl the most important facts which anatomy has brought to li respecting the structure of the brain in illustration of functions.

Taking the simplest and most general view of the struct of the several parts of the cerebro-spinal nervous system its relation to their functions, we may look upon the wh arrangement as one fitted to bring the external or periphe organs for the reception of sensory impressions, and t production of motions, into connection with the cent organ or brain, and ultimately with its superficial coveri of grey substance in which its highest functions are elaborat This is effected by a succession of conducting channels fibrous nerve substance, and connecting or interrupti masses of grey matter which lie between the opposite t mini of the system, and which may be brought under t three following categories, viz.:—1st, Those which are ne the peripheral organs, comprising the sensory and moto nerves, all of which are connected at their roots with gr substance in the spinal marrow or brain; 2nd, those whic intervene between the nerve roots and the central cerebr ganglia, including the cerebral and cerebellar peduncles c stems, and the deep masses of grey substance which occup the ganglia and other situations in this division of the sy tem; and 3rd, the spreading fibres which, emerging from th ganglia and deeper parts of the brain, expand in the cerebra hemispheres till they reach and combine with the grey sub stance of the surface and its convolutions.

1. With respect to the first division of these parts, as th

description of the structure of the nerves does not belong to my present subject, I will only remark that, assuming that the root-fibres of the nerves are all connected with grey substance at their central origin, and that none pass directly inwards to the brain without such intervention, we may regard this grey matter to be the means not only of connecting together nerve fibres of different endowments, so as to favour their mutual interaction, but also of transferring nervous influences from or to the next set of channels of communication between the brain and the peripheral organs.

2. The second set of parts in this system, situated between the grey matter which gives origin to the various nerves and the deeper parts of the brain, differ very much in the spinal marrow and in the brain itself; consisting, in the first, of comparatively simple tracts of white fibrous substance, but in the latter attaining much greater complexity, as they are prolonged onwards through the medulla oblongata, and are reinforced by new additions in the pons Varolii and cerebral and cerebellar peduncles, before they reach their upper limits in the grey substance of the cerebral ganglia.

In the spinal marrow the whole of the white substance by which nervous influences are mainly carried upwards, is arranged so as to form the anterior, lateral, and posterior columns, in all of which (and especially in the lateral) the direction of the nerve fibres is mainly longitudinal. Although doubts still prevail among physiologists as to the exact course taken by sensory impressions and motor influences through the cord, it may be stated as most probable that motor influences pass downwards in the fore-part of the lateral columns, and perhaps also in part in the anterior columns; while sensitive impressions are conveyed upwards in the hinder part of the lateral columns, and perhaps also in part in the internal grey substance and in the posterior columns. And it may be stated, that in these last-mentioned parts the nerve fibres are on the whole much smaller than in the fore-part of the lateral, or in the anterior columns.

Here, too, must be noted the important fact resulting from physiological experiment—that, while motor influences descend through the columns of the cord on the same side as the roots of the nerves upon which they act, sensory im-

pressions (at least those of pain and touch) ascend on the opposite side from that on which the posterior or sensory roots have entered. Thus it appears that the most of the fibres in communication with those of the sensory roots cross at once to the opposite side of the cord, as may indeed be seen in the grey commissure, while the motor fibres remain on the same side. But at the upper part of the spinal marrow, or rather when they have just entered the medulla oblongata, the motor columns suddenly cross, in more or less divided bundles of fibres, from one side to the other. This occurs in the so-called decussation of the pyramids, which consists essentially in the oblique passage across the middle plane of a considerable part of the lateral columns of the cord, containing its chief motor fibres, into the opposite anterior pyramids of the medulla oblongata. Thus, as is well known from pathological observations and physiological experiments, the effect of deep-seated injury of the brain on one side is always manifested by more or less widely-spread palsy,—that is, loss of motor or of sensory power, or of both, on the opposite side of the body.

The increase and modification which the ascending columns of the spinal marrow undergo as they are prolonged into the medulla oblongata, may be shortly explained as follows :—

a. The anterior spinal columns, being thrust aside by the intervention of the pyramids, become the olivary columns of the oblong medulla, and contain within them the dentated nucleus of the olivary body, the cells of which appear to be a source of new fibres, which pass across between the bodies of opposite sides, and from each also into the cerebellar peduncles of the opposite side. The main part of the olivary columns passing deeply through the pons Varolii are continued up into the corpora quadrigemina as the fillet. The properties of this nervous tract are not well known; but, seeing that the anterior spinal columns are intimately connected with the motor nerve roots, and the corpora quadrigemina with vision, a conjecture may be hazarded, that through them are regulated the motions of the body which are dependent upon guidance by sight.

b. The anterior pyramids, which are the prolongations of the decussated motor part of the lateral spinal columns,

ascend through the pons Varolii at a small depth from its anterior surface, and form the lower part, or crust, of the crura cerebri. Thence they penetrate for the most part the corpora striata, and are believed to be connected with its grey nuclei, more especially the nucleus lenticularis. It is doubtful whether any of its fibres pass into the cerebral hemispheres without having been previously united with grey substance.

c. The upper part of the cerebral peduncle differs greatly from the lower in having its fibres much intermixed with grey substance. It consists mainly of fibres prolonged upwards from a part, probably the sensory part, of the lateral column of the spinal marrow, together with a portion of the posterior column, and it is generally regarded as a channel of sensory conduction. Along with the foregoing, it receives from behind an accession in the processus a cerebello ad cerebrum, and a part of the combined fibres may pass into the corpora quadrigemina lying immediately above the crura cerebri.

d. The remaining parts of the spinal medullary columns, consisting of a large portion of the posterior, together with offsets from the lateral and anterior columns, enter into the formation of the restiform body or inferior peduncle of the cerebellum, into which its fibres spread towards the surface, along with the other fibres which enter that body from its superior or cerebral peduncles and from the pons Varolii. The last-mentioned body, which appears to be developed in proportion to the size of the lateral masses of the cerebellum, is composed mainly of transverse fibres as it enters the cerebellum and forms the middle peduncles of that body on the two sides. Crossing from side to side below, and imbedding the longitudinal columns which pass from the medulla oblongata into the crura cerebri, it appears at first sight to be mainly a transverse commissure of the cerebellum; but its fibres are much intermingled with grey substance, and undergo decussation in such a way that they are considered to establish communication between the cruri cerebri of one side, and the opposite side of the cerebellum.

The several parts of the great cerebral stalk or peduncle may be considered to terminate in or lead to the various

masses of grey substance occupying the interior of t
so-called cerebral ganglia; the white fibres of the crust,
lower part, proceeding most directly to the corpus striatu
those of the tegmentum, or upper part, to the thalami ar
corpora quadrigemina; and it is now generally believed th
the greater number of the motor fibres are collected in tl
crust, while the tegmentum contains a preponderance of se
sory fibres. In connection with this view, it is also held, o
pathological as well as on physiological grounds, that th
corpora striata are the great centres of motor impulses; th
thalami and corpora quadrigemina the principal seats of th
arrival of sensory impressions, and of their reaction on th
motor centres of all the automatic actions.

It is indeed certain that animals deprived of their cerebra
hemispheres are still possessed of some sensory faculty, an
capable, under excitement, of many purposive movement
of great complexity, even when a part only of the cerebra
ganglia remains entire, although they are altogether withou
voluntary power or spontaneity of action.

3. The third and highest division of the parts of the brair now under consideration comprises the large mass of medullary fibres which extend from the upper confines of the cerebral ganglia into the covering capsule of grey matter by which the convoluted surface of the cerebrum is everywhere surrounded. This forms the corona radiata of authors, and has the form of a fan or brush-like expansion of fibres, which stretch from the outer and upper sides of the corpora striata and optic thalami into the centres of the convolutions, penetrating into and intimately uniting with the superficial grey substance.

Upon the supposition that the will and other mental operations are exercised in connection with the active condition of the superficial covering of grey matter, it is conceived that the peculiar hastate or spear-like corpuscles or cells which occupy this substance in several successive layers, and with which undoubtedly the terminal nerve fibres are in some way united, are the more immediate seat of the nervous action by which these operations are evolved. But we must confess ourselves profoundly ignorant as to the nature of this action and of the share very probably taken in it

y the different structural elements which are associated in he composition of the grey substance.

For the production of conscious sensation or perception, ve may suppose that the sensory impressions which have een carried from the periphery as far as the thalamus, or ther great sensory centre, should be there transferred to ertain fibres of the corona radiata, and conveyed by them o some part of the superficial grey substance in which they roduce determinate changes; and that, for the formation of deas and judgments, there shall occur the various combinaion of such conscious perceptions, with the psychical changes ollowing on the consequent cerebral actions in one or other art of the covering grey layer of the brain.

These perceptions, ideas, and judgments leave certain impressions on the structural elements, by the action of which hey have been evolved, which, though in abeyance when hese elements are not in an active state, are capable of being e-excited or recalled in the process of memory.

So also, under the influence of present conscious perceptions, ideas, and judgments, as well as of their reproduction by memory, there may be excited, in connection with one or other part of the cerebral cortex, those impulses to which we give the name of will, which, when directed outwards on the motor mechanism of the ganglia and nervous channels, lead to all our active movements by the induced contraction of the muscles.

We are, however, still ignorant whether these faculties and powers result from an action which pervades the whole or any part indifferently of the superficial cerebral substance, or whether it may be possible to localise their various forms as depending upon the nervous activity of distinct portions of the brain. Although the well-known phrenological system has failed to produce the proofs satisfactory to physiologists which its votaries anticipated for it, there are many facts ascertained from pathological observation, and from the recent experimental researches of Hitzig, Ferrier, and others, which indicate with great probability that certain regions of the superficial grey covering of the brain are more immediately connected than others with the excitement of motions and the reception of sensations; and the view gains ground

that there are also certain parts of the cerebral cortex wi which the several psychical processes are specially related.

The fibrous parts now described as extending in two stag between the motor and sensory nerves on the one hand•a the surface of the brain on the other, have been named t peduncular system, to distinguish them from two other se of fibres which form important constituents of the cerebr structures—viz., the commissural and the collateral sets fibres.

The commissures consist of fibres which, passing acro the middle plane, bring into union and consentaneous actic corresponding parts on the right and left sides of the brai The largest mass of this kind is the corpus callosum, c great cerebral commissure, the fibres of which accompan those of the corona radiata in their expansion into the hem spheres and their convoluted surface so closely, that it i almost impossible to distinguish between the peduncular an the commissural fibres after they have become associate together. The anterior and posterior white commissures an the grey commissure similarly unite opposite parts of th hemispheres, and the pons Varolii is probably, at least i part, a commissure of the two lateral halves of the cerebellum

Thus, too, it appears that the brain, as regards its mos superficial part and highest psychical operations, is in some measure a double organ, while the action of the cerebra ganglia is almost entirely unilateral.

There are several remarkable pathological observations which prove that the mind may still remain entire notwith- standing the removal of large portions of the upper part of the brain of one side. The history of the remarkable affec- tion termed Aphasia is a good illustration of the cross and the unilateral action of a part of the brain.

In the great majority of cases of this morbid condition, while the persons retain more or less, and sometimes very perfectly, their knowledge of the meaning of words as well as their general intelligence, they have lost the power of expressing ideas in speech, without, however, any impairment of the power or control over the muscles of the organs of voice or articulation, or even the ability to repeat words spoken by another; while, at the same time, they are affected with

paralysis of one or both limbs, and that almost always on the right side of the body. Now, in these instances, according to the discovery of the French pathologist, M. Broca, the lesion or disease of the brain is found to be situated in the back part of the third or lowest frontal convolution, just at the entrance to the fissure of Sylvius. And thus in the affection of Aphasia we have combined a unilateral superficial cerebral lesion, with opposite unilateral palsy of the body, and an interference with a process which consists more immediately in the excitement of a bilateral motor mechanism for the conversion of ideas into spoken or written language: and yet, in some strange and inexplicable manner, this mechanism seems to be excitable only from one side, while the upper part of the brain, so far as the ideas and the will are concerned, seems to be capable of working equally by either side.

The connection between different parts of the brain on the same side is established by means of the third set of fibres to which I have referred—viz., the collateral longitudinal or associating, the nature and uses of which are even less known than those of the other two sets previously described.

The fornix is one of the parts belonging to this system, and seems to connect the hippocampi of the middle and posterior lobes with the thalami optici, through the curious twisted course of its anterior pillars in the corpora albicantia. The taenia semicircularis, the longitudinal striae of the corpus callosum, the fibres running along the convolution of that body, and the uncinate fasciculus, are other instances of bands of fibres confined to one side, and serving apparently to unite different parts of the brain, but in a manner and for purposes which are still wholly unknown. The fibres which pass from convolution to convolution in the bottom of the grooves between them may be brought under the same division, and may be looked upon as the probable means of connecting together different parts of the grey covering of the brain, of combining, therefore, possibly sensations, ideas, voluntary impulses, or other psychical operations which may belong to different regions of that part of the cerebral organization.

Lastly, the expanded portion of the cerebellum, which

attains a considerable size in man and the higher animals, presents a structure in some respects comparable to the convoluted part of the cerebral hemispheres. A layer of grey matter, containing cells of two kinds, covers the whole outer surface, and is thrown into inflections of a plated or foliated form, so as greatly to increase its extent, just as is caused by the convoluted form in the superficial part of the cerebrum. The interior of the plates is occupied by an extension of the fibrous white substance formed by the expansion of the combined fibres of the three peduncles before referred to, and somewhat in the same manner as in the cerebrum, the fibres of the deeper white substance of the cerebellum enter into communication with the larger branched cells of the grey substance.

By means of the lower peduncle fibres are carried into the cerebellum from the several columns of the spinal cord, those of a sensory kind appearing to predominate; by the superior peduncles the fibres of the cerebellum are made to join superiorly the tegmentum of the crus cerebri, and perhaps also the corpora quadrigemina; while the pons Varolii seems to unite the opposite halves of the cerebellum below, and to establish other unions between its stem and the roots of the cerebral and spinal nerves.

The cerebellum does not appear to exercise any important function in connection with sensation, volition, or intellect, but experimental evidence indicates that it has some close relation to the co-ordination of voluntary movements. The mode of its operation, or the relation of its structure to this or any other function, is still whollv unknown.

In the preceding slight sketch of the minute structure of the brain, from which I have been obliged by want of time to omit all details, I have endeavoured to show how intimately the various processes which make up our sentient, percipient, active and intelligent nature are associated with an organization of the most delicate, complicated, and refined description; and how closely the advance in the development of nervous and mental powers follows the advance in

the perfection of structure in the bodily organ, in the first formation of the being and its subsequent progress to maturity, as well as in all the varying conditions of animal life. I by no means wish to be understood to contend that our knowledge, either of nervous phenomena or of the structure of nervous organs, is yet such as to warrant dogmatic assertion of an indissoluble and direct relation between organization and mind; but I venture to affirm that, if we deal with this problem in the same manner as with other scientific investigations, we cannot arrive at any other conclusion than that mental processes, however complicated they may become in their higher forms, have taken their first origin in nervous action resulting from the vital activity of nervous structure, and that their rise into higher and higher forms of psychical phenomena is only a fuller development and closer combination of repeated and more complicated nervous actions.

I would here remark that, although a mechanism be of organic nature, and subject to the complex and varying conditions of a living organization, the phenomena which result from its activity are not less the immediate product of its internal changes than are those of the evolution of heat, light, electricity, and magnetism, from the bodies with which these forces or states of inorganic matter are observed to be associated. We do not know nerve force as distinct from the nervous fibre. We have good reason to believe that by some modification of that force in its passage through the nerve-cell, an afferent nervous impression is converted into an efferent impulse in the phenomenon of reflex action. And it does no violence to our power of conception to extend the same view to the more complex mechanism situated within the cerebral ganglia, by which all those motions which we style automatic appear to be regulated without the co-operation or control of will or intelligence. When, however, the same afferent impression, which causes a simple reflex or a more complicated automatic motion, reaches the higher part of the brain and results in sensation or perception, what grounds have we, on any physiological or scientific principle, for asserting that the change which follows is other than a higher manifestation of some nervous property?

As physiologists we know of the exercise of mind only in its association with the brain, as a part of our organization in a state of integrity, and in the conditions of life ascertained to be necessary to the manifestation of its action. Surely the intelligent part of our nature, if in itself noble and elevated, can suffer no degradation from its corporeal origin being ascertained. Rather might we say that the corporeal organization gains in dignity by the proof of its association with the nobler principles of our nature; and that the sciences which profess to investigate the relation between cerebral organization and mental phenomena, thus become entitled to the highest rank among the attainments of human knowledge.

The dignity or elevation of any branch of knowledge will obviously depend mainly upon the character given to it by those who inquire into it; and if, upon sufficient evidence, the unbiassed judgment of science arrives at the conclusion that the operations of the mind are the result of the vital activity of cerebral organization, it neither alters in any respect the real essence of mind as distinguished from matter, nor does it detract in the least from the greatness of its powers. While, on the other hand, the careful study and increased knowledge of the relation between mind and body may be the means of making us better acquainted with the nature of our mental powers, and may lead to useful practical improvement in their cultivation. We cannot doubt, at all events, that a healthy organization, and an observance of sanitary laws of life, are necessary to the evolution of the best mental efforts; and it is no less certain that hereditary influence is equally powerful with educational culture in carrying our mental powers to their highest state of perfection.

GENERAL PHYSIOLOGY OF THE NERVOUS SYSTEM.

LAST year you will remember that Dr. Allen Thomson delivered a lecture in this hall upon what he very properly called the architecture of the brain and of the nervous system. The lecture which I propose to deliver this evening may be regarded as a continuation of the lecture given by Dr. Allen Thomson. You will recollect that in that lecture he pointed out to us that the nervous system consists, in the first place, of certain cords found in almost every part of the body called *nerves*, and of certain masses of matter found in other parts of the body, to which he gave the name of *nerve centres*. These nerve centres, you will recollect, he classified as *ganglia*, or smaller masses of nervous matter found in certain parts, and the *brain* and *spinal cord* constituting what he termed the cerebro-spinal system. You will remember also how he showed us that the nervous system, when traced from the simplest forms in the lower animals to its more complete state in the higher animals, gradually becomes more and more complex from the development of new parts. He illustrated this by taking us through the different groups of the vertebrate kingdom of nature, and showed us how, from the simple brain, as found in the fish, we might pass step by step, to the brain of the reptile, of the bird, up to the brain of the mammal; and at last he described the architecture of the brain of man, which is the most complex of all. He illustrated the same general principle of evolution, from the simpler to the more complex forms, by tracing the development of the brain in a single individual, from its earliest form in the embryonic state to its full completion in the adult. In order to refresh your memories in regard to one or two of these points, so that you may be better able to comprehend what I have to say regarding

another aspect of the subject, I may direct your attentio in the first place, to one or two of the diagrams which I Thomson showed us on that occasion. This large diagra represents a side view of the brain of man. Underneat we have a diagrammatic or typical brain; and I wish y to keep in mind that it consists essentially of the f lowing parts :—First, of a large mass (bounded by dott lines, in the lower diagram), representing the *cerebrum* in fully-developed brain. Behind this, and overlapped by it, the higher brain, we ha a mass of nervous matt called the *corpus striatu* Behind this, again, a som what smaller mass, to whi the name of *thalamus o ticus* has been given—a old name given by anat mists, who thought th this part of the brain w more specially connecte with the sense of visio Behind the *optic thalamu* we have a still small mass of matter, which w shall call the *optic lob* specially connected wit the nerves of vision. B hind this, again, we have continuation of the brai in the form of *crura*, c *peduncles*. Then the uppe part of the spinal cord c marrow called the *medull oblongata*. Lastly, we fin the spinal cord itsel

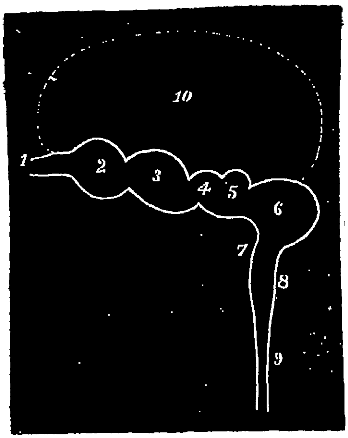

Fig. 1.—Diagram of an Ideal or Typical Brain. 1, Olfactory lobe; 2, cerebrum; 3, corpus striatum; 4, optic thalmus; 5, optic lobe; 6, cerebellum; 7, pons Varolii; 8, medulla oblongata; 9, spinal cord; the dotted curve indicates the possible development of the cerebral lobes.

These are the parts, the names and general position of whic it will be important to carry in your memory, so as t understand what may be hereafter said as to their uses.*

* The woodcut here introduced, although not a copy of the di gram actually shown at the lecture, illustrates this paragraph.

Let me direct your attention to several other diagrams constructed by Dr. Allen Thomson. This diagram gives us a general view of brain-development, from the brain of fish upwards to the fully-developed brain of man. This other diagram gives a general notion of the arrangement of the spinal marrow or spinal cord. You will see that the cord consists of a series of columns of white matter, placed side by side, and enclosing a mass of grey matter in the centre. From the sides of the spinal cord, and from the base of the brain, we have numerous nerves originating, which course or run through various parts of the body. These are the principal anatomical points which I think you should remember in order to follow the physiological side of the question.

This evening, I now ask you to look at the nervous system, not from the anatomical side, nor from the point of view of the comparative anatomist, but from that of a physiologist. The science of physiology has for its object the investigation of the *functions* of organs. The science of anatomy, of course, has, strictly speaking, to do with the *structure* and *form* of organs, and also, I think, with the *evolution* of organs, as traced throughout the animal kingdom. When we examine the different forms of animal life, we first of all notice that in the lower orders there is no trace of a nervous system. There are many microscopical organisms, and many organisms quite visible to the naked eye, which have no nervous system. And yet, if we stimulate or touch these organisms, we may cause them to move. One of the first and most remarkable phenomena of life with which we have to do is *movement*. Many minute and almost structureless animals move by contracting—by changing their form—and this they may accomplish without any nervous system whatsoever. As we proceed higher in the scale, we come to animals where a nervous system begins to be apparent. It has been generally supposed that the nervous system first made its appearance in the form of little masses of nervous matter connected with two nerves. Quite recently, a number of most important observations have been made upon *medusæ* by Mr. Romanes, who has paid a great deal of attention to these interesting animals. You are all

familiar with the forms of the beautiful medusæ we s floating in the sea. These creatures are sometimes clear a transparent as glass. They move by rhythmic contractio and expansions of certain parts of the body. Mr. Romar found that they might be experimented upon, and that th moved or made rhythmical contractions when stimulated certain points. He discovered that, if you touch one these medusæ, or irritate it, say by an electric current, a s of wave will be transmitted from the point irritated towa the centre of the animal's body, or perhaps to some p ticular part of it. He also found that if he took a la medusa, such as we find in the Moray Frith, and cut into the form of a long ribbon, and irritated one end of ribbon, a movement or wave of contraction passed to other end. He observed that if he cut the ribbon half-w across, cutting from each side of the ribbon, as it were, as to leave simply little strips connecting its segments, t still, on irritating one end of the ribbon, the movem passed from the point of irritation towards the other e All this indicated what might be regarded as a beginn or differentiation of a nervous system, in the sense that th were definite paths along which the stimulus might tra More recently, as I have been informed by letter, Mr. Schä of University College, London, has been investigating structure of some of these medusæ on which Mr. Roma experimented, and he has found indications of a nerv tissue. Thus, in these simple creatures, whilst there are nerves, there are what may be called "lines of nervous for —lines along which a special influence passes which res in contraction. These lines of tissue, in higher forms of may gradually become more differentiated into what we u mately term nerves.

The next step we notice is when we have to do v *nerves*, *ganglia*, and *centres*. What is the structure a *nerve?* A nerve, when seen with the naked eye, prese the appearance of a white cord. When examined under microscope, it is found to consist of an immense numbe fibres. These little fibres, when seen individually under microscope, are clear and transparent like glass, but w looked at attentively, they are found to have definite pa

This figure represents a view of a nerve fibre diagrammatically. It consists of a delicate sheath outside, within which there is a cylinder of matter—which we call the *white substance*, but which in the diagram I have coloured blue, simply for the purposes of illustration. In the centre of the white substance, we have semi-gelatinous material (coloured red in the diagram), which we may term the *axis rod*, or *axis cylinder*. Suppose we make a section of a single *nerve fibre*, what is here represented in blue, is the white substance, surrounding the axis or cylinder rod. A nerve is made up of an immense number of these little nerve fibres laid side by side.

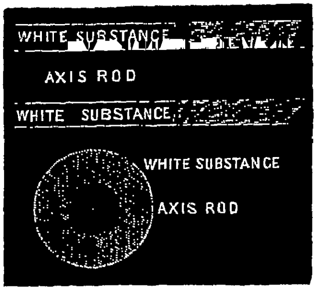

Fig. 2.—Diagrammatic View of the Structure of a Nerve Fibre. Above, longitudinal; and below, transverse section.

This diagram represents a portion of a section of the optic nerve of a horse. The nerve was hardened, and a thin transverse section made of it, which was stained with colouring matters employed in histological work. We see a portion coloured red, which consists of a kind of connective tissue binding the whole together. Then you have enormous numbers of nerve fibres. In this section

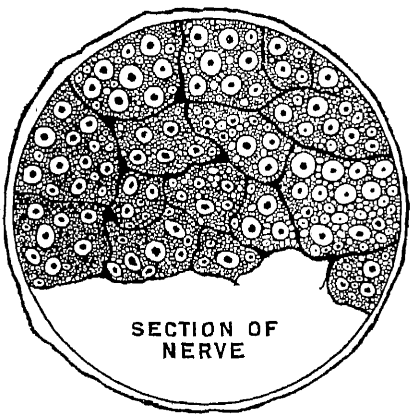

Fig. 3.—Transverse Section of a Nerve, showing the Bands of Connective Tissue and Sections of Nerve Fibres.

you see them cut across. The axis rod is represented as a dot; and in the section of the nerve you have the nerve fibres of different diameters, some of them being very much greater than others. That, then, is the general structure of a nerve.

Now, let us ask ourselves next what is the function of a nerve? Perhaps the best way we can illustrate that is to imagine that we can submit it to some kind of experiment. We cannot learn anything of its functions by simply looking at it. We might learn something about it by tracing it, and finding out where it goes. If we found it went to a muscle, we would naturally conclude that it had something to do with muscle; if it went to a blood vessel, we would conclude it had something to do with blood vessels; or if it went to the brain, we would conclude it had something to do with the activities of the brain. Suppose we irritated it in some way, say by a feeble electrical current, various things might happen which would indicate what its functions may be. In the first place, we might have a *contraction of the muscle* —the muscle supplied by the nerve. When the nerve is irritated, this muscle might contract. The various movements of locomotion and other voluntary movements are carried on in this way. When we lift the arm, a nervous impulse travels along certain nerves from the brain or cord to the muscle, the result being a muscular contraction. But, instead of muscular contraction, we might have possibly *secretion from glands*. Thus there are nerves which supply the salivary glands; and if we irritate any of these nerves, there may be an increased secretion of saliva. Among the many nerves in the body, there can be no doubt at all that there are certain ones which excite secretion. But, again, instead of movement—or contraction of the muscles—or secretion of glands, we might have a change produced upon the *diameter* of *blood vessels*. The little blood vessels which ramify through the body, carrying their supply to the organs and tissues, are not tubes of the same calibre at all times; but they are tubes having contractile walls, and these walls are also under the influence of nerves. On irritating some nerves, we might see the calibre of the vessel becoming much narrower; while, on irritating other nerves, possibly

the calibre of the vessel might become much larger. We thus might have, on irritating a nerve, contraction of muscles, secretion from glands, or possibly a change in the diameter of vessels. Then, fourthly, on irritating the nerve, there might be indications of feeling. In that case, the nerve must have passed, in some way or other, to a sentient brain. Nerves then carry impressions from the periphery of the body to the centres, and when they reach the sentient centres (of which I shall have to speak presently), the result is a sensation or feeling.

These are four modes of nervous activity, which we may notice in almost any animal, and which occur in man. In some animals, however, nerves supply other structures. For example, here is a diagram in which you see represented a peculiar fish, the torpedo, an inhabitant of the Mediterranean. This fish belongs, as many of you know, to the family of the Rays or Skates. Its general formation and appearance are shown by the diagram. You will observe that we have laid bare the nervous system and ganglia of which I have been speaking. You will see numerous nerve cords passing from the nerve centre and distributing themselves to very remarkable looking organs shaped somewhat like a honeycomb. These are electrical organs. If you touch this creature, placing one hand on the back of the fish and the other on the belly, you get a severe

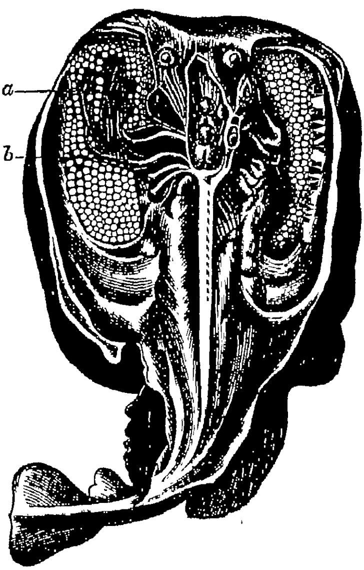

Fig. 4.—TORPEDO, showing the Brain, Spinal Cord, and Nerves; also the Electric Organ supplied by large Nerves, *a*. The Electric Organ is the honey-comb like structure, *b*.

electric shock. It is interesting to know that the discharge of electricity from this electric organ depends upon the activity of the nervous system. When the electric organ is examined closely, it is found to present a very close analogy to the structure of the voltaic pile; that is to say, if you take different metals, separate them by little bits of some bibulous materials, and immerse the whole in some solution which will act on one of the metals, you obtain a current of electricity. In the torpedo, each of these little hexagonal bodies may be regarded as a little pile similar to a voltaic pile. It has been ascertained by careful experiment that the nerve is distributed to the *positive* surface of this pile. The interesting point in connection with the physiology of the torpedo is, that in it nerve action may produce changes in tissue which result in a discharge of electricity. We have thus an *electrical phenomenon* dependent on nervous influence.

Consequently, to recapitulate, a nerve, on being stimulated, may produce motion or contraction of the muscles, or it may produce sensation or a feeling of pain; it may produce change in the calibre of the blood vessels, secretion from the glands, or, possibly, some electrical discharge from a special electrical apparatus, as in the case of the torpedo.

In the next place, suppose you proceeded to examine the nerves going to the different parts of the body. The first thing that would occur to you is, that you would see no visible difference in the nerves; they present the same appearance (except in size), so that you could not tell one from the other, although stimulation of them may produce such diverse effects. Nor can you make out any distinctions even with powerful microscopes, as all the nerve fibres look alike. This leads us to the important inference that the different effects produced by stimulating different nerves does not depend on the structure of the nerve, but upon the nature of some apparatus at its end. The nerve is then really a *conductor*. When stimulated, it displays a mode of activity which we may term nerve force or nerve energy; this travels towards a termination, and the effect produced depends upon what we may call a *terminal organ*, or the apparatus at the end of the nerve. You will understand this perhaps by taking an

nalogy from electrical phenomena. A copper wire conducts current of electricity, and this current may be caused to o very different kinds of work. For instance, the current ight be employed to do mechanical work by driving a ttle electro-magnetic engine; it might also be used for producing chemical decomposition, as in electrotyping; or it ight be used for producing the phenomena of light, as in e electric light; or it might, by altering the condition of a nall magnet, cause a little disc to vibrate, and thus produce und, as in the telephone. You will observe that the copper ire will conduct the current to any of these different kinds apparatus, but the difference of effect really depends upon e apparatus at the end of the wire. Now it is similar in e case of the nerve. The nerve, as I have said, may be garded really as a conductor—a conductor of some change other in the nerve—and the effect produced depends upon e nature of the apparatus at the end. If the apparatus be cell or cells in a sentient brain, the result will be a feeling pain, or perhaps of pleasure. If the nerve terminate in electrical organ, it will be an electrical discharge; if it d in a muscle, it will be a muscular movement or contracn. This important generalization has been the result, as u will quite understand, of a great deal of physiological vestigation. At one time it was supposed that the different nerves had different properties, and that what was led a *motor* nerve, or one which causes motion, was different in structure from a *sensory* nerve, which, on being itated, might cause a feeling of pain. We now know t that is not the case. The nerves are similar, but the ct depends upon what is at the end.

The nerve, you will observe, is thus a conductor; but at same time it is something more than a conductor. Supe you were to expose a nerve in any part of its course, l irritate this nerve in any way at that point, at once you up some kind of activity in that nerve which travels ng the nerve. Consequently, a nerve is not only a contor, but it is an *excitable* conductor—it is capable of ng excited by an external stimulus, applied at any point. this respect, you will see that it differs from an electrical luctor. It is capable of being touched or stimulated or

irritated at any part of its course. Many of you must no doubt have been struck with the remarkable analogies between nervous action and electrical action. The very appearance of a transverse section of a nerve (see fig. 3) suggests the idea of a cable having a number of wires for the conveyance of currents of electricity; and if you can imagine this cable branching off here and there to go to different towns or places, you have at once something remarkably similar to a nervous distribution. But you may make the analogy even more close. If you look at the transverse section of a single nerve fibre, it suggests to one some kind of an insulated structure. When the electrician wishes to keep the wires from contact, so as to prevent the passage of currents from one wire to another, he covers the wire with some non-conducting substance. Now, when you look at a nerve fibre, you see an axis-rod surrounded by the white substance. We have no proof that this white substance is really an insulator; in fact, it is not an insulator of electric currents, at all events—and we have no absolute proof that it is an insulator of the energy which passes along the central rod. But you say, what reason have we for supposing that the central rod is a conducting structure? This view is founded principally upon histological evidence, that is, the evidence got by microscopical examination. If we trace a nerve fibre to its termination, we find it ultimately terminates by the central rod connecting itself with the terminal apparatus. For example, if you trace a nerve into the spinal cord, we find that it may end in what is termed the pole of a nerve cell in the grey matter of the cord. You find that the central rod comes into absolute organic contact with these cells. The white substance disappears before the central rod comes into contact. Again, if you trace nerve fibres into connection with the terminal apparatus in the eye—the retina—or with the terminal apparatus of the ear, by which we appreciate sound, you find illustrations of the same truth—that the central rod is what is connected with the terminal organ. Consequently, the central rod is, in all probability, the conducting structure.

There are many points of difference between what we may call electrical conduction—or conduction of electricity along

a conductor—and nervous action. One of the most remarkable facts is that the nerve current passes along a nerve with comparative slowness. It does not travel at anything like the rate of electricity, for instance, along a copper wire (288,000 miles per second—*Wheatstone*).

One of the triumphs of modern physiology has been the measurement of the rapidity of the nerve current. If you move the finger, the movement, you think, is instantaneous. If you *will* to move a finger, the finger is apparently moved the instant you will that the thing is to be done. No time seems to be lost. This results from the fact that our appreciation of minute intervals of time does not pass beyond a certain limit. We cannot appreciate very minute intervals of time. We cannot observe a period of time of less, say, than one-tenth of a second. If two phenomena happen, separated by a shorter interval than the one-tenth of a second, they seem as one to our consciousness. Many phenomena, however, may occur, and do occur, in a much less space of time than that. I have had a diagram constructed to show how physiologists have measured the rate of a nerve current. There are many experiments which cannot be done in such a hall as this, and therefore we shall have to be contented with what we see in the diagram. Take the muscle of a frog having a nerve attached to it. We stimulate this nerve, laying it across two wires, A and B. The problem is to find out how long the nerve current takes to pass from A to B., etc., in fig. 5. The velocity of the nerve current in a *motor* nerve was first determined by Professor Helmholtz, of Berlin.

Various modes of measurement have been adopted by physiologists from time to time, but the general principle of all the methods is to cause a muscle to contract by successively irritating two points of a nerve more or less distant from it, and to obtain a record of the contracting muscle upon a rapidly moving surface. It is evident that the distance between the two points at which the two curves leave the horizontal line will indicate the length of time the nerve current took in passing from the stimulated point, at a distance from the muscle, to the other stimulated point close to the muscle. A convenient arrangement for making

the experiment is shown in this diagram (fig. 5), in which an instrument termed the spring-myograph of Du Bois-Reymond is employed.

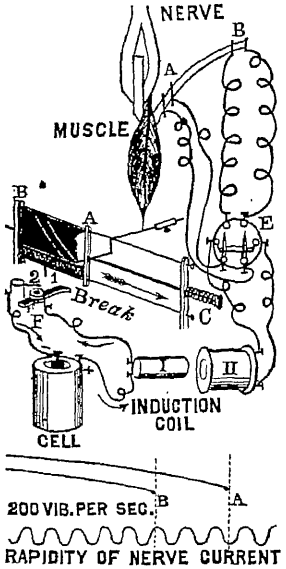

Fig. 5.—Diagram showing arrangement of Apparatus in experiment for measuring the rate of the Nerve-current.

The apparatus consists of a smoked glass plate, which is driven in front of the recording stylet of the myograph by the recoil of a steel spring C. Underneath the frame carrying the glass plate, there are two binding screws, 1, 2, to one of which is attached a rectangular arm of brass, 1, which can so move horizontally as to establish metallic connection between the two binding screws (marked *break* F in the diagram). By means of these binding screws, the myograph is interposed in the circuit of a galvanic cell, and the primary coil (I) of an induction machine, and the arm of brass is so placed as to connect both binding screws, and thus complete the circuit. From underneath the frame carrying the smoked glass plate there descends a small flange which (when the glass plate, by releasing a catch not seen in the figure, is driven across by the spring or spiral at C from left to right) pushes the brass arm aside, and thus interrupts the circuit of the primary coil. When this occurs, an opening shock is transmitted from the secondary coil, II, to a commutator, E, an instrument by which electric currents may be transmitted in one direction or another at pleasure. By means of the commutator, the secondary shock may be transmitted to the nerve, either to a point close to the muscle at A, or at a distance from it at B. Suppose the apparatus all arranged, so as to send the shock to the nerve at a point close to the muscle, A, the muscle thus

stimulated contracts, and draws, by the stylet, on the smoked surface of the glass, the curve A, seen in the lower part of the figure. This curve leaves the horizontal line (which would be drawn by the muscle at rest) at A. Arrangements are then made for another experiment, in which the nerve will be stimulated at a distance from the muscle, at the point B, in the upper part of the diagram. This is done by again placing the smoked glass plate in proper position, closing the primary circuit by the brass arm at the binding screws, as already described, and reversing the commutator so as to send the shock along the wires to B. The muscle again contracts when the primary circuit is opened, and this time it describes on the smoked surface the curve B, seen in the lower part of the diagram. It will be seen that this curve leaves the horizontal line at B, that is, a little *later* than when the nerve was stimulated close to the muscle. It follows, therefore, that the distance on the horizontal line from A to B represents the time occupied by the transmission of the nervous impulse from B to A of the nerve. With suitable arrangements, the rate of movement of the glass plate can be measured, by bringing a vibrating tuning fork into contact with it. The vibrations of the fork being uniform as regards time, the little waves thus recorded enable us to measure with accuracy the rate of movement of the glass plate, and consequently the minute interval of time between A and B. In the diagram, it will be observed that there are two waves between A and B; each represents $\frac{1}{200}$ of a second, and $\frac{2}{200}$ of a second, or $\frac{1}{100}$, is the time represented by the length of the line from A to B.

When this experiment is carefully done, it is made out that, in the frog, the nerve current travels at the rate of about 90 feet per second. That is comparatively a slow speed. By other methods, which I have not time to describe, the rate of the nerve current can be measured in the living man without any great trouble; and it has been found that in man, and probably in other warm-blooded animals, the current goes at the rate of about 200 feet per second. I need not say that that is incomparably slower than the passage of a current of electricity along a wire (see p. 13).

The next point I wish to direct your attention to is, what

is it that really passes along the nerve? That is an extremely difficult question to answer. In fact, at this moment it cannot be answered satisfactorily. It is not electricity, in the sense that it is often spoken of. It has been supposed by some to be a kind of molecular movement passing along the nerve. Some have supposed that it was an instantaneous chemical change produced at the point of irritation, and that this chemical change was transmitted very quickly from point to point along the nerve. From various observations which I have made, I am inclined to think that, when a nerve is irritated, a change passes simultaneously to both ends of the nerve. Formerly, we were in the habit of supposing that, when we irritated a nerve at one point, the current went only in one direction. But there are a number of facts to show that, when we irritate a nerve at any particular point, the change goes in both directions. You say, How is it, then, you have not an effect in both directions? The answer is, because it will depend on the nature of the apparatus at the end of the nerve. When we irritate a nerve, as I said before, an effect will be seen only at the end at which there is a responsive terminal apparatus. No apparent effect will be observed at the other end, but there may be transmission in that direction notwithstanding. Imagine a long india-rubber tube filled with water lying on the table, and that at one end of it there is a little lever laid across it. On tapping the tube smartly in the middle, a wave will be propelled towards *each* end of the tube, but its existence will be shown only at *one* end by the movement of the lever. So is it, I think, with a nerve. Irritate a nerve in any part of its course; from its structure, and from various experiments, I infer that an impulse travels to both ends, but direct evidence of this impulse will be given only at the end in contact with a terminal organ.

Next, what occurs at the termination of the nerve? That we may illustrate very well by taking the example of muscular contraction. Here we have a muscle fixed in a little apparatus, and a nerve stretched across two wires. We have attached the muscle to a long lever, which you see; and when we irritate the muscle, you see the lever lifted up; and if you have a weight attached to this lever, you cause the

muscle to do work in lifting the lever, and in lifting a certain weight. An extremely feeble current is quite sufficient to irritate a nerve, and consequently the amount of work which a muscle does in lifting a weight does not depend on the amount of stimulus which you apply to the nerve. You set the nerve in action, and it conveys some kind of stimulus to the muscle, which has the result of liberating the energy stored up in the latter. The muscle may be regarded really as containing energy in a potential condition, and the action of the nerve is not merely to force the muscle to contract; but, as it were, to set free this energy in the muscle. The change is manifested by a contraction. The relation of the nerve to the muscle is something similar, for example, to the relation of the trigger that acts upon the percussion cap, and explodes a great mass of gunpowder. The gunpowder contains, of course, an immense amount of energy which is locked up or latent. You liberate this energy by setting up some kind of action by means of the percussion cap, and instantly the liberated energy will do work. In the same way, in nervous actions, a very feeble primary irritation is sufficient to produce a great effect. The muscles contain energy stored up in themselves, and the nerve may be regarded as the liberator of the energy. In like manner, the brain and nervous centres generally may be regarded as stores of energy which we have obtained from food, and the action of the nerves is to set up changes in these parts, so that energy is set free. The energy thus set free may be manifested in various ways, as we have seen.

We next pass on to consider the *nerve centres.* These are made up of nerve cells and nerve fibres. I have already said enough about nerve fibres. Nerve cells are of many kinds. They are microscopic in size, and they present many varied forms. I exhibit one of Dr. Allen Thomson's diagrams, and you see a vertical section of the grey matter on the surface of the brain. In this matter, observe the peculiarly formed nerve cells. Each of these nerve cells has minute processes or poles proceeding from it, and each pole is connected either with a similar pole from another nerve cell, or with the axis cylinder of a nerve. You can trace, as I have said before, the axis-rod into con-

nection with these cells. You see in this diagram two or three very large cells of quite a different form, such as you might obtain from the grey matter in the centre of the spinal cord of an ox or any large animal. A single nerve cell, with two poles or processes, may be regarded as the simplest kind of nerve centre.

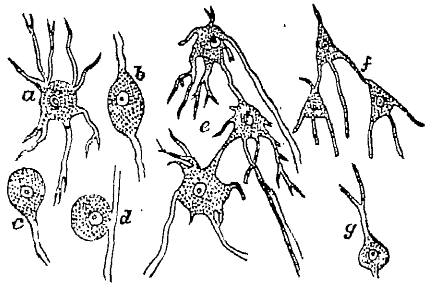

Fig. 6.—Various Kinds of Nerve Cells. *a*, *Multipolar*, from grey matter of the spinal cord; *b*, *d*, *bipolar*, from ganglion on posterior roots of spinal nerves; *c*, *g*, *unipolar*, from cerebellum; *g* shows indications of a process coming off at lower end; *e*, union of three multipolar cells in spinal cord; *f*, union of three *tripolar* cells in grey matter of cerebral hemispheres.

In discussing the physiology of the centres, I wish to proceed according to the same plan as Dr. Allen Thomson, who passed from the simpler to the more complex forms. Probably the simplest nervous mechanism we can conceive consists of a cell and two fibres. In this diagram, you have a large cell, and attached to this cell you have a fibre conveying an impulse inwards. This fibre I have coloured red, and the direction of the impulse is indicated by an arrow. When

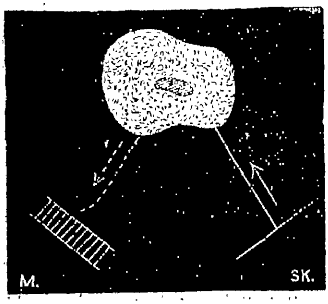

Fig. 7.—Diagram showing simple reflex arrangement. M, muscle; SK, skin.

the influence reaches this cell. a change occurs in it,

which results in the transmission along the other fibre of an impression outwards, and when this impression reaches a muscle it excites a contraction. This is an automatic arrangement, and is usually termed a *reflex action.* Touch the skin of an animal of a very humble form, and immediately you have a contraction. To aid the mind in understanding the mechanism, examine the diagram again. The impulse travels along the red fibre (from S K), and comes out at a blue fibre (towards M). The red is the sensory nerve carrying impressions inwards, and the blue one is a motor nerve carrying impressions outwards. The term reflex was first given to an automatic nervous action by Dr. Marshall Hall, but I think it is a very unfortunate one. Reflex, of course, gives one the idea of something reflected —some kind of influence passing into a centre, and then thrown out from the centre, just as a ray of light striking upon a surface would be reflected from that surface. I do not think that is the kind of action. Instead of having it arranged as in that diagram, you may imagine, for instance, the cell to be placed in the length of the nerve fibre. You may have a fibre, for example, as represented in this drawing, and a cell in the centre of the fibre. When you irritate the one end of the fibre, the impression would travel along to the cell, through the cell, and along the fibre again. This cell might have had three processes instead of two, and in that case the cell would distribute the influence which it received from the first fibre. Suppose, for example, the cell (fig. 7) had another fibre passing upwards. The impulse coming in at the red or sensory fibre might then be partly sent to the muscle along the motor fibre, or sent away upwards to another cell, perhaps in the brain. Nerve cells, then, may be regarded as arrangements for generating or for reinforcing the nerve current. Suppose I take this galvanic cell and charge it with acid, and then carry the electric current along wires to an electrical engine, so as to make it work. If I place another cell in the circuit somewhere, this second cell will strengthen the current, and the engine will of course work faster.

Now, in the case of a nervous mechanism, the change which would take place in cell *a* when the end of the fibre

passing to it was irritated might be transmitted to cell *b*, which would reinforce the effect, so that you would produce a much greater effect by such a mechanism of two or more cells than by one only. Thus a simple reflex mechanism may become much more complex. For instance, we may have several fibres, instead of one fibre, passing to the cell; or, instead of one cell, we may have half-a-dozen cells all grouped together and connected with various fibres. Thus we would have what we might call a *complex reflex mechanism.* Again, on tracing the development in the higher forms of life, we find that there is a tendency to what may be called a grouping of reflex centres. In the humbler forms of animal life, we have probably reflex centres here and there throughout the organism, but by-and-by you have them clustered together into masses. That is very well seen in the nervous system of an insect. In it you have certain larger masses; one, for instance, in the head, which is essentially connected with the organs of sense. Again, in the thorax and abdomen, we find other groups of reflex centres massed together so as to form ganglia. This tendency to aggregation becomes more and more marked as we ascend in the scale of animal life, until we find its highest development in the cerebro-spinal system of man.

The *spinal cord* is to be regarded in two aspects: as a conductor, and as a reflex centre. In the first place, the outer part of it consists of nerve fibres. The anterior part consists of fibres which carry impressions downwards from the brain to the muscles. The posterior part consists of fibres which carry impressions upwards from the periphery of the body to the brain, or to the grey matter of the cord itself. You see in the transverse section, in the lower part of this diagram, the grey matter in the centre of the cord, which contains numerous nerve cells. This grey matter in the centre of the cord may be regarded as consisting of numerous reflex centres—centres connected with various movements of the limbs and of other parts of the body. Reflex movements are not usually associated with consciousness; they are merely automatic. The molecular change, or whatever it may be, which occurs in the

nerve cell never reaches that state of activity which results in consciousness or sensation.

In studying reflex mechanism in animals and also in man we sometimes observe that there is what we may call a diffusion of reflex movements; that is to say, if you set up an irritation in a lower reflex centre, this irritation may pass to other centres, and consequently give rise to more general diffuse movements.

This may be illustrated by reference to the diagram, fig. 9, p. 29. Here we have a lower reflex centre (1), say in the spinal cord, connected by two nerves with muscle M and skin SK. On irritating the skin, the influence may affect only one muscle or group of muscles, or it may be partly diffused or transmitted to a higher centre (2), so as to call into action other groups of muscles. Still higher centres may also be involved, so that the contraction of many muscles may be the result of an irritation applied to the skin.

I may be allowed here to refer to a practical question with regard to reflex movements. I have noticed recently in our newspapers that a discussion has taken place in regard to a very important subject, and one which is somewhat disagreeable in its details—the modes which we adopt of killing animals. I have noticed that it has been objected to one of these modes that violent convulsive movements took place, when, after the animal had been felled, a cane was thrust into the spinal marrow; and in some of the letters it has been assumed that these movements implied consciousness—that is to say, that the creature was actually conscious of pain at the time it made these movements. In the particular method I refer to, after the animal is knocked down with one blow, a rod or cane is pushed into the medulla and the spinal cord. That, of course, is done with the view of completely destroying the animal's power of feeling pain, although I believe consciousness will be entirely abolished by the blow alone. But that is precisely the kind of procedure that would create the violent convulsive movements which have been referred to. If you pass a rod or wire into the spinal cord of the animal which has been killed, you will excite violent spasms. But these spasms are no indication of consciousness; they are simply

the result of intense discharges of nerve-energy from the irritated cord, violently excited by the passing of the wire or other substance into the interior of the spinal canal. In connection with the matter, let me remark, that everything possible should be done to prevent pain in the killing of animals. What physicians and surgeons and physiologists have to fight against is pain of every kind, whether occurring in man or in any living creature however humble. Therefore we should direct our thoughts carefully and attentively to devise those methods which may be best suited for suddenly depriving the creature of life, if we hold that we are warranted in depriving it of life to meet our necessities.

To return to our proper subject, let me now direct your attention to a higher stage in the scale of nervous action, and to study what is known of sensation. All the arrangements I have up to this time described may be quite unassociated with sensation, and usually are so. They may occur, of course, along with sensation, but sensation is not necessarily involved. Let me explain this by taking an example. Suppose you see a child sound asleep. In profound sleep, the person is in a state of absolute unconsciousness. Imagine that he is in such an unconscious state; that there are even no dreams. If you tickle the sole of the foot or touch it, the foot is drawn away, the body may be turned over, and numerous movements may occur. These are reflex movements, unassociated with consciousness. Suppose you tickle the sole of the foot of a man who has met with an injury to his spine, which deprives him of the power of voluntary movement, and that even then some movements occur, these movements are, in the circumstances, beyond the control of the person to resist them. He does not *feel* that you touched the sole of his foot, and still the movements occur. We know, from what I have said before, that these movements depend on reflex centres in the spinal cord. But now, go a step higher, and we come to certain centres which are connected with sensation—with a feeling which may be of pleasure or of pain. I must here define what I mean by a sensation, because I find in many books—more especially in some popular books—that the term is used in an exceedingly loose way. Some writers, for instance,

in speaking of the action of a reflex centre, which I have just been describing, would use the term in this way :—A sensory nerve carries an impression to the centre; in the centre a *sensation* occurs—and the result is something transmitted along the motor nerve, causing motion. To apply the term sensation to such a change in a nerve centre is not correct. A sensation must be associated with a conscious state, or rather, it is a conscious state; we can have no sensation unless we are, as it were, conscious of the sensation. It is the *consciousness of an impression:* it is a conscious condition, resulting from some kind of irritation applied to the periphery of the body, and of some subsequent changes occurring in the brain. But what is the mechanism of a sensation? What arrangements in the body are necessary for it? They appear to be three in number. *First,* we must have what we call a terminal apparatus for receiving an external stimulus; *second,* we must have a nerve for conveying the change thus produced; and *third,* we must have a sentient brain to receive the message. We can illustrate this by what occurs in the process by which we become conscious of light. Light outside of us may be regarded as a movement, just as sound is a movement, and when it acts on the retina of the eye, it excites there some kind of activity; this activity irritates the extremities of the optic nerve; the impression is carried to the brain, and we become conscious of the impression. That is a sensation of light. Imagine a person sound asleep. Gently open the eye and put a light before it. The nervous apparatus will then act, no doubt, but the effect on the brain is not perceived. Whatever change may occur in the brain in these circumstances, it really does not rise to the condition of what we may call a luminous impression. Thus, even with a light before the eye, the sleeping person does not *see.* The same is true with regard to all the other senses.

Each sense has its own terminal organ, and that terminal organ is specially fitted for the reception of a particular kind of external stimulus. By means of our senses we thus derive information regarding the external world. Outside of us there are various movements—the movements of sound, the movements of light, molecular movements, and those delicate movements which result in what we call chemical change.

We have terminal organs specially fitted for the reception of most of these movements. When the nervous apparatus works, these movements become transformed in our consciousness into sensations. For example, the retina is a structure no doubt specially arranged for being acted upon by light. Some recent interesting observations by Boll and Kühne have shown that the action of light upon it is something very analogous to what happens when light acts upon a sensitive photographic plate. Again, in the case of sound we have in the ear a special terminal apparatus for vibrations of sound. Thus the movements ultimately irritate the auditory nerve, and the impression is a sensation of sound. The necessity of a terminal apparatus does not seem at firs very evident. You say, Why should not the external energy or whatever it is, act directly on the end of the nerve? bu physiology has shown us that it does *not* affect the nerve a all. A terminal organ seems to be absolutely necessary. Thus, there is on the retina of the eye a spot which i not sensitive to light. It has been long known as th blind spot. It is the part where the fibres of the opti nerve are bare—that is, where there is no proper retina structure. Consequently, the fibres of the optic nerve to be excited by light, must be excited through the ter minal organ. The same is no doubt true of the othe senses. These nerves which carry impressions of light, c sound, of taste, of touch, are usually termed nerves c *special* sensibility; and the reason is, that in whatever wa you irritate one of these nerves of special sensibility, yo always produce the same kind of effect. Thus, when yo irritate the optic nerve by pressure, by an electric curren by light on the retina, or chemically, or by cutting it, th effect *is always a luminous impression.* That depends upo the principle I have already laid down: the result is owin to what we have at the end of the nerve. The message goe along the optic nerve, and when it reaches the brain, it ther excites changes which are always interpreted by the mind a a luminous impression.

Up to this point, you can imagine an animal sentient; tha is to say, conscious of certain sensations of light, of sound, c

of some of its wants, and conscious also of what you might call feelings of the body — impressions transmitted from various organs, which give rise to a sense of satisfaction or of the reverse. This animal might also be capable of moving, it might have an electrical organ, or it might have secretory organs, but still there might be nothing of the nature of an emotion, except it might be a feeling of pain or of pleasure. But such a creature might have no ideas or thoughts, or power of restraint or of volition. Therefore, as we rise higher in the scale, we come to a still more complex nervous arrangement, namely, one for regulating the other centres. One of the most interesting steps in modern physiology has been, I think, the discovery that certain nerves, instead of stimulating to contraction, have the reverse effect; that is to say, they stop contraction. This was first discovered in the case of the heart by Weber, a German physiologist. It was well known that the heart of some animals, say the frog, will pulsate for a considerable time after it has been removed from the body, and that these pulsations go on in consequence of the heart having little nerve centres of its own. The heart of the frog and of the higher animals, however, is supplied by two great nerves, which connect these intrinsic centres with the cerebro-spinal system. Both of these nerves come from the medulla, the upper part of the spinal cord. Weber's discovery was this—if you irritate in the neck the great nerve called the pneumogastric or vagus, the heart beats more slowly, or it may stop its contractions. Fibres of this nerve find their way to the heart. Now, observe that you have not caused a spasmodic contraction on stimulating the nerve, but the very reverse. If the nerve be cut, and the lower part irritated very gently, the heart will move more slowly; remove the irritation and it will go on as before. The heart is thus under two nervous influences. One goes along the pneumogastric, causing it to go slowly; the other along the sympathetic, causing it to go faster. The heart is spurred on by the one and restrained by the other. It has recently been shown, moreover, that in the heart itself we have a centre which also restrains its activity. Such nerves or nerve centres as restrain the activity of other nerves or of other nerve centres, are called *inhibitory*.

In the accompanying figure (fig. 8), showing the nervous mechanism of the heart, the ganglion connected with the sympathetic is marked A, that in connection with the pneumogastric (inhibitory) I, and the third, controlled by the other two, R.

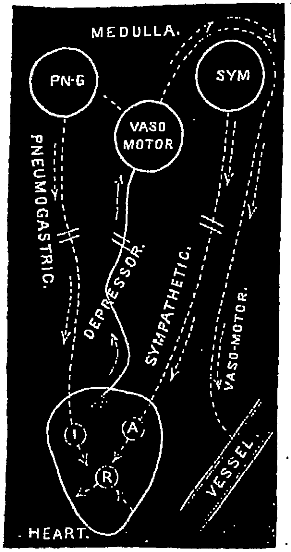

Fig. 8.—DIAGRAM SHOWING ARRANGEMENT OF THE HEART AND VESSELS. I, Inhibitory centre; A, accelerating centre connected with sympathetic through spinal cord; R, reflex centre.

Regarding the mechanism of inhibition we know nothing definite. It is possible that the effect may be the result of throwing in some kind of resistance at the reflex centre, so as to prevent it working quickly, or perhaps the nervous arrangements are such that there may be some kind of short circuiting of the nervous currents; in other words, a mechanism by which it is possible to carry away the ordinary current which keeps up the activity of the heart. Such an arrangement of sending the current along another channel would prevent the heart from working, and cause it to stop. There are many other examples of inhibition, some of a most interesting character. I may be allowed to refer to one other, which will show the wonderful influence which the nervous system has upon all our functions. I show you an apparatus which is intended to illustrate some facts regarding the circulation of the blood. It consists of two glass cylinders, placed vertically, and communicating with each other by a horizontal glass tube. Let the one vertical tube represent the arterial system and the other the venous, and let the horizontal tube represent the system of capillaries which connects the end of the arteries with the commence-

ment of the veins. Suppose now that by means of this syringe or pump, which may regarded as the heart, I force water into the arterial system; you will observe that the pressure is at once increased in this system, so as to be greater than in the venous system, but that the two soon become equal by the fluid in the arterial system emptying itself into the venous. This is exactly what is the state of matters in the living body. When the heart contracts, it throws blood into the arterial system, and consequently the pressure is much greater in the arteries than in the veins. The arteries, however, empty themselves into the veins through the capillaries; and if the heart did not again contract, the pressure in the arterial and venous systems would be equalised, and the circulation would stop. In some circumstances, the arterial circulation may be such that the heart must overcome a great resistance in forcing the fluid onwards. Suppose, for instance, you had a contractile structure, or pump, forcing the water through the streets of a town, and that from some cause or other the pipes became very much contracted in some parts of the town, so as to offer great resistance to the force of the pump; the resistance might become so great that the pump would be unable to drive the fluid onwards, and the pump itself might, in its attempts to overcome the resistance, be permanently injured. What naturally would suggest itself to the mind of an engineer? He would remove the resistance by widening the pipes at the contracted portion. We have somewhat analogous arrangements in the body. The blood-vessels are under the control of a system of nerve filaments in the sympathetic, termed *vaso-motor*, by the action of which on their muscular walls the calibre of the vessels is regulated. These *vaso-motor* fibres originate in the *medulla oblongata* at the base of the brain, from a spot termed the *vaso-motor centre*. It has recently been shown that the action of this centre may be inhibited (or partially thrown out of gear, as it were), by the action of a set of filaments in the pneumogastric, which carry impressions upwards from the heart to the brain. These filaments, in some animals, form a slender nerve, distinct from the pneumogastric below a certain point, now called the *depressor* nerve (see fig. 8). The action of this

nerve is probably as follows: Suppose, for example, that th smaller arteries throughout the body were in a state of suc contraction that the blood could not pass quickly throug them. In these circumstances, the large vessels woul become distended, the tension of the blood on their wall increased, and the heart would have to labour hard to forc the blood onwards. When this condition occurs, an influenc may be sent from the heart, along the *depressor* nerve, t the *vaso-motor* centre in the medulla, the effect of which i to inhibit this centre. The instant the centre is inhibite the smaller blood-vessels relax, the blood flows more freel through them, the tension or pressure in the larger vessel is diminished, and the heart, having less resistance in fron works more easily. Thus we must regard the blood-vessel as a system of living tubes, subject to such influences of th nervous system as the requirements of the tissues and of th heart demand.

Many other examples of inhibition might be given. Ther may be unconscious or conscious inhibition. The inhibitio acting on the heart, for instance, is unassociated with co sciousness. We are not conscious of any such mechanism a I have just described. But in rare circumstances, I believ we may be conscious of it. There was a physiologist i Germany, named Czermak, who died some years ago, an who could compress the pneumogastric in his neck. H noticed that when he did so the heart beat more slowly, an he had a feeling of intense constriction in the region of th heart. He said it was precisely the same kind of feeling c constriction which we sometimes experience along with great grief, and this may explain to us how it is that em tional states of our higher centres, acting downwards, ma influence the heart directly. So that the popular notion c emotional states acting directly on the heart is physiologicall true.

Another step in the development of the nervous syste is the setting aside of centres having functions necessar for life, such as to carry on the mechanism of respiration an the process of swallowing. These great centres are situate in the medulla oblongata. You will see them represented i the following diagram. Some of the nerves connected wit

these centres are distributed to the lungs, some to the heart and vessels, and some to the digestive organs. All these arrangements are for the purpose of effecting reflex movements, of which we may or may not be conscious, but which we cannot restrain. Thus we cannot restrain breathing for almost any time; the desire to breathe becomes unconquerable. So with regard to the calibre of vessels, and consequently the distribution of blood; and also with regard to swallowing, and the movements of the digestive organs. The *medulla oblongata* is often spoken of by French physiologists as a *vital* part, the *nœud vital.* They imagined that at this point life existed more than at any other part of the body. That is not the true way to put it. If we destroy this part of the nervous system we destroy life, in consequence of these great functions necessary to life being at once stopped, but vitality is not greater at that part than at any other.

The exact position of the centres indicated in the diagram, fig. 9, may be seen in fig. 10.

We have now supposed these centres added to other centres, until the

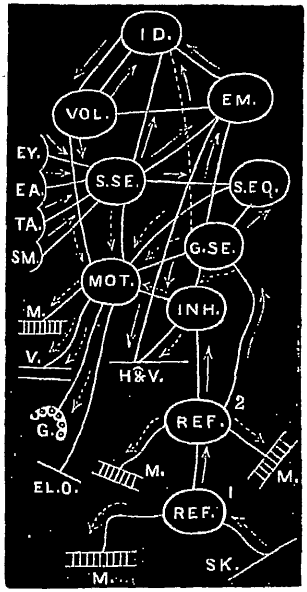

Fig. 9.—Diagram showing various kinds of Nervous Actions. M, muscle; SK, skin; REF, reflex centre; INH, inhibitory centre; MOT, motor centre; G.SE, centre of general sensation; S.SE, centre of special sensation; S.EQ, sense of equilibrium; VOL, volitional centre; EM, emotional centre; ID, ideational centre; EY, eye; EA, ear; TA, taste; SM, smell; V, vessel; G, gland; H&V, heart and vessels. The arrows indicate the direction of the currents. By following the directions indicated by the arrows, the influence of one centre over another may be studied.

mechanism becomes very complex. Imagine, further, that a creature has many muscles for moving different organs, and that these muscles are more or less connected together by reflex centres. You will see at once the necessity for what we may call the *co-ordinating system*. This co-ordinating system exists in the lower brain or cerebellum marked with the letters CO (fig. 10). Let me explain what I mean by co-ordinating. For instance, if I lift this match-box by finger and thumb, and flex my arm, I have caused a number of muscles to contract more or less. These contractions have had to be precisely regulated with regard to each other so as to produce this definite effect, but I do all this unconsciously. Influences are sent out along many nerves to these different muscles, and the result is a co-ordinating movement. This co-ordination of movement is carried on to a much larger extent, of course, in playing or using a difficult instrument, as in the movements of the hand of a performer on a piano. The movements of the muscles require to be co-ordinated. This is accomplished principally by the agency of the *cerebellum.*

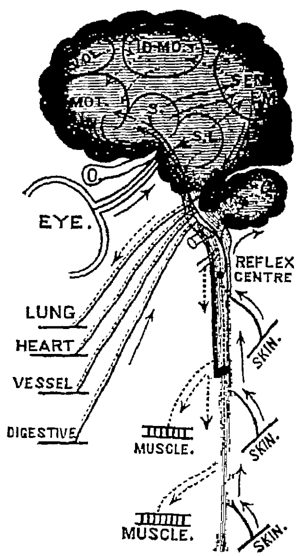

Fig. 10.—DIAGRAM OF CEREBRO-SPINAL AXIS, showing position of various centres. CO, co-ordinating; SL, sense of light; S, sensory; MOT, motor; SEN, sensory; EM, emotional; VOL, volitional; ID-MO, ideo-motor; arrows indicate direction of current.

Many facts could be brought before you in support of this view, but one of the most important, perhaps, is that which you can see illustrated in this diagram, fig. 11. Suppose this represents a pigeon, from which the cerebellum has been removed. There is no loss of consciousness. It is able to

take its food; all its senses are intact; but it is unable to fly and unable to walk—not because it is paralysed, but because it is incapable of *regulating* its movements in a definite way. When it attempts to flap both wings, it may move one wing inwards and the other outwards; it does not co-ordinate the movements, so as to produce the desired effect.

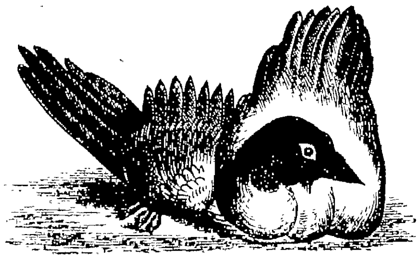

Fig. 11.—PIGEON, in which the Cerebellum has been injured or removed.

We pass upwards to the higher centres in the brain, and finally to the brain itself. There are the *corpora striata*, specially connected with movement, any injury to which will cause loss of movement on the opposite side of the body. The reason why the effect is on the opposite side is illustrated by Dr. Allen Thomson, as you see in the diagram, where you observe the motor fibres crossing at the top of the cord. The *optic thalami* are not connected with the sense of vision, as was at one time supposed, but are centres for receiving sensory impressions passing upwards. These sensory impressions may pass beyond these into cerebral centres, where I believe consciousness actually occurs. Behind the optic thalami we have the *optic lobes*,* which receive the fibres of the optic nerves. The optic lobes act as reflex centres connected with sight. In illustration of this, I might take a phenomenon which may sometimes be seen in a person

* The parts termed optic lobes are in the higher forms of brain subdivided still further; each lobe is divided into two, and the *four* bodies thus formed have been called *corpora quadrigemina.*

in a somnambulistic state. In this condition, the eyes of the person may be fully open, but he is quite unconscious or is wrapped up in his own dream. If, in these circumstances, a light is brought before the eye, the person does not push his head against the light. He avoids the light. He moves to one side, as if he were actually conscious of the movement. The stimulation by light seems to excite a movement unconsciously, and a movement is therefore made to avoid the light. Here the optic lobes probably act simply as automatic reflex centres.

The *cerebral hemispheres* consist of a layer of grey matter over white matter in the centre. The grey matter is raised in convolutions.

Flourens and the older observers were aware of the fact, that as successive slices of grey matter were removed from the surface of the cerebrum, an animal becomes more and more dull and stupid, until at last all indications of perception and volition disappear. A pigeon in this condition, as depicted in fig. 12, if carefully fed, may live for many

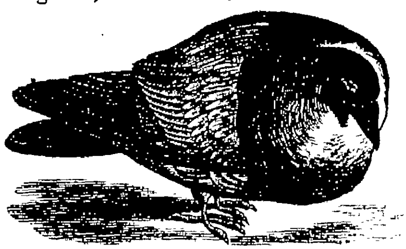

Fig. 12.—Pigeon, in which the Cerebrum has been injured or removed.

months. The following description, given by Dalton, is so accurate as to merit quotation:—"The effect of this mutilation is simply to plunge the animal into a state of profound stupor, in which it is almost entirely inattentive to surrounding objects. The bird remains sitting motionless upon his perch, or standing upon the ground, with the eyes closed, and the head sunk upon the shoulders. The plumage is smooth and glossy, but is uniformly expanded by a kind of erection of the feathers, so that the body appears somewhat

puffed out and larger than usual. Occasionally the bird opens its eyes with a vacant stare, stretches its neck, perhaps shakes its bill once or twice, or smoothes down the feathers upon its shoulders, and then relapses into its former apathetic condition." Similar observations have also been made on reptiles and mammals, but the latter survive the operation for a comparatively short time. In watching such an animal, it is difficult to divest one's mind of the idea that it still feels, and hears, and sees. It may be observed, however, that it makes no movement unless stimulated from without. Thus, it may remain motionless for many hours; but, if pushed, or even gently touched, it will then move. It manifests no fear even when placed in circumstances likely to excite it, and it will walk over the edge of a table, or into the fire, quite regardless of consequences. There is thus no proof that it experiences any sensation in the sense of consciousness of impressions; or, in other words, there is no perception. As well remarked by Michael Foster: "No image, whether pleasant or terrible, whether of food or of an enemy, produces any effect on it, other than that of an object reflecting more or less light. And, though the plaintive character of the cry which it gives forth when pinched, suggests to the observer the existence of passion, it is probable that this is a wrong interpretation of a vocal action; the cry appears plaintive, simply because, in consquence of the completeness of the reflex nervous machinery, and the absence of the usual restraints, it is prolonged. The animal is able to execute all its ordinary bodily movements, but in its performances nothing is ever seen to indicate the retention of an educated intelligence."

The method of removing a portion of the brain is open to many objections, the chief of which is, that the severity of the operation and the loss of blood may cause such a state of shock, as to vitiate any inferences that might be drawn from the facts recorded. A new method, however, has been devised, namely, that of stimulating the nervous centres by electricity, and observing the results.

It was supposed by the older physiologists, that the whole of this great mass of brain matter was connected with what we may term *mental phenomena*, and had nothing to do with

motor phenomena. But recently some observations have been made which would appear to indicate that certain parts of the grey matter on the surface of the brain are connected with motion. During the Franco-Prussian war, a French soldier had the misfortune to have a portion of brain exposed, and two German surgeons, Fritsch and Hitzig, had occasion to stimulate the surface of the brain feebly with an electric current. It was supposed, up to this time, that the surface of the brain was quite insensitive to a current of electricity, but these surgeons observed a movement of the eyeballs. That was the beginning of an important research carried on by Fritsch and Hitzig in Germany, and by Dr. Ferrier in London, which has given us a vast number of facts regarding the functions of the grey matter over the central region of the brain. If the grey matter in the central region of the brain be irritated at certain points with a feeble current of electricity, the result is a definite movement of some part of the body. For instance, Dr. Ferrier found that when he touched a particular part, the right fore-limb was extended; when he touched another part, the left fore-limb; another part, the right hind-foot, and so on, as if there were a number of centres definitely arranged for these movements. Dr. Ferrier called these centres *idio-motor centres*, supposing that they had probably to do with what we may call an idea which results in a movement. I am inclined to think that many of the movements observed by these physiologists are really of a reflex character, that is to say, when you irritate an area of grey matter on the surface of the brain, impressions are carried down to a motor centre, perhaps in the corpus striatum. That is a matter, however, upon which I have not time to dilate. If you irritate the anterior part of the brain, as has been pointed out by various physiologists, you tend to restrain reflex movements, which have their centres in the spinal cord. I am quite sure, from my own observation, that that is the case. If you irritate this part of the brain, some reflex movements may be restrained or interrupted. Thus the anterior part of the cerebral convolutions may be supposed to contain inhibitory centres, which may act either on the motor centres in the middle region of the brain, or on the motor centres in the cord. The back

part of the brain is, I believe, principally for sensation. I have put in here also in the diagram, fig. 10, the emotional centres. Some have imagined that emotional impressions may result from activity in the ganglia at the base of the brain. I think the evidence in support of that statement is very insufficient.

Such, then, is what I may call the general mechanism of the nervous system. You will understand it better, perhaps, by referring again to the diagram (fig. 9), in which the arrows represent the direction of the nervous currents. Here first are the lower reflex centres (1 and 2). These may act upon the higher reflex centres. If an influence travel up to a higher centre we have a general sensation. This sensation, which may occur in an animal low in the scale, may act on motor centres, and these motor centres may cause muscles to work. I have introduced here an inhibitory centre, which might restrain the activity of the motor centre. Then we have the emotional centre, which might receive influences from a centre of special sensation, or from a centre of general sensation. Highest of all, we have the volitional centres and ideational centres.

I have now got to the end of my subject. I should like to show one experiment which may illustrate to you what might possibly be the kind of action which takes place in some nervous arrangements. It is very remarkable that a very feeble stimulus originated in the brain, is quite sufficient to produce very powerful spasmodic effects. A man wills to perform certain movements, and instantly you have a number of muscles acting together. You can scarcely imagine that energy exciting all these movements really originates in a particular part of the brain, and is the same energy which carries it to a termination. I have often thought it not at all improbable, that in nervous mechanisms, we may have something of the nature of what a practical electrician would term a system of relays. That is to say, you can have a feeble current of electricity—rendered feeble by passing along a distance—too weak to work a machine, but quite sufficient to close a local circuit, getting new force from local cells, and thus carrying the current onwards. If that arrangement does not exist in the nervous system, then

another method might be adopted. The same current might be sent alternately in different directions, and caused to do different kinds of work. That I have spoken of already. Here I have fitted up an experiment which suggests, as it were, what I mean. I have here a current of electricity passing into an arrangement by which I can send it to work an electro-magnetic engine, or to work two tuning-forks, or to cause a telephone to sound loudly, and, if the current were strong enough, it might produce light. That experiment shows how one may have many different effects produced by one current.

The last point I wish to refer to is with regard to the question of the connection between mental phenomena generally and activity of brain. On the one side we have undoubtedly the fact of the dependence of mental phenomena on brain. That is generally admitted. Assuming that this is the case, we know that if we interfere in any way with the brain, we interfere with the mental state of the person. This is illustrated by the very well-known and sad facts of insanity. At one time insanity was supposed to be a diseased mental state. In one sense it is; but we know perfectly well that it is also a bodily disease; and I would take this opportunity, as I have often done before, of pressing this aspect of the subject upon the public. It is now generally admitted by all well-informed persons, although there still lingers a superstitious notion that insanity is something different from a bodily affection. When this view held ground—that insanity was merely a mental state, not connected with the body—it led, as many of you know, to very wrong systems of treatment of the unfortunate insane. But when men found out that there can be no form of insanity without some kind of disorganization—some morbid change or other in the grey matter of the surface of the brain—then there was a guide to the treatment of such cases, in the direction likely to cure the sufferer.

For instance, we at one time supposed that nothing morbid was discovered in the brain of the insane; but histological examination has shown us that traces of some change or other are always to be found. The whole of this subject is by no means worked out. It is indeed still in its very infancy;

but we have quite enough evidence to go upon to satisfy us that in insanity we have to deal with a bodily disease; and consequently all the efforts of humane physicians engaged in the treatment of the insane are directed towards placing them in as favourable a condition as they possibly can be for recovery. They treat them upon the same general principles as they would attempt to treat disease in any other organ of the body.

Another proof of the fact that mental phenomena depend on the brain, is found in what we notice with regard to nutrition. If we interfere in any way with the quality of the blood going to the brain, the effect is apparent. We know, for instance, that the introduction of alcohol into the blood produces a most marked change. If you can imagine a person under the influence of alcohol, in a state of intoxication, and that condition to be permanent, you would justly regard that person as in an insane state; that is to say, the activities of the brain are altered by the introduction of this matter into the system. The reason why I allude to this is to show how intimately mental states depend upon states of blood and brain. We must all think of mental states as depending mainly upon brain and on blood—the kind and quality of blood, and the arrangement for getting rid of the waste products of the brain, so as to facilitate those chemical changes which result in brain activity. On the other hand, we have many phenomena connected with mental activity which at present have no physiological explanation. Such are the phenomena of the emotional states, and the facts of memory and of intellectual action. We simply know that these states are connected with a particular part of the brain, but we have no knowledge whatever of the changes which result in the part of the brain connected with these states. The mechanism is quite unknown; and I believe myself it is very likely that it may be for ever hidden. It is impossible to bridge across what may be regarded as the gulf between the *objective*—the chemical and physical operations occurring in the grey matter of the brain—and the *subjective*, or mental states which we associate with these. We cannot really think of the one, as it were, passing into the other. The mind

fails in attempting to grasp the connection between th two. Suppose, for example, that in the future this ma be the case—that we had examined every bit of the surfac of the brain, and knew precisely where every nerve cell wa placed, and how these various nerve cells were connecte together; suppose we knew all the chemical changes occur ring in this part of the brain, and how the impulse wen from nerve cell to nerve cell: would we be any nearer a understanding of how these actions produced particula mental conditions? I do not see that we could possibly b any nearer. We would simply be able to say that menta conditions were associated with such and such occurrence and such and such chemical phenomena. It has alway appeared to me that it was useless to require of physiologist an explanation of the relation between mind and matte They cannot give it. It is one of the "ultimate truths" c their science. Nor need it be imagined that this is th *only* insoluble problem of physiological science. For exampl the beginning of life is just as mysterious as its close. Whe we examine an ovum or egg—a minute microscopic particle c living matter—who can explain by what molecular arrange ment it is that it contains all the potentiality of a futur being? And if you go to the department of physical o chemical science, you will meet with the same difficultie There are ultimate problems in physical and chemical scienc which are really just as obscure in many ways as what w may call the ultimate problems of physiology. I have ofte thought that the workings of the brain and other so-calle vital phenomena are connected with the very highest pro blems of chemistry and of physics. Further, we are ver far from having explored its minute anatomy. We kno absolutely nothing of the chemical changes occurring in th brain—and chemical changes are constantly occurring ther —the co-incident phenomena of which are our states c mental activity. There have been many opinions formed b thinkers as to the connection between mind and brai Some have supposed the brain to be like an instrument pe formed upon by some invisible agency; others have conje tured that you have the two things—brain action and min action—going on and working side by side.

I think by far the safest view to take of the subject is to put it in this way: In our present phase of existence there is a most intimate connection between mental phenomena and physical states of the brain. You cannot have mental phenomena, in our present state of existence, without some operations occurring in these higher centres which I have been speaking about. That is a totally different thing, however, from saying that these mental states are simply the product of these physical operations, and that when these physical operations cease the mental states cease also.

In this particular region we pass from the domain of science into the domain of belief, dependant upon another kind of evidence. I think that that is the only legitimate way of meeting the great difficulty. We pass from the field of science—from what we know or from what we infer from certain facts—into what I might call the field of hope and faith; not faith simply as blind acceptance of what we are told, but a faith associated with hope, which is as real as if we had a demonstration of facts.

Apart from these speculations, which will probably be repeated in diverse forms by man to the end of time, there are certain practical aspects of this subject which I have brought under your notice to-night. First of all, we have the general fact that mental states are affected by physiological conditions. We have our physiological conditions more or less under our own control, therefore we have our mental state more or less under our control. In this way, physiologically, every man has the power to make or mar himself. He cannot practise any vice, he cannot indulge habitually in anything that interferes with the nutrition of the brain, without affecting the physical organization of this important part of his body; and when he affects the physical organization of this part, he must inevitably affect his mental qualities. That is what might be called a very materialistic way of putting it, but I think it is true. You cannot possibly affect your nervous organism without leaving certain impressions upon it which will alter you from what you were before.

The study of nervous physiology also indicates, I think, the importance of training, or developing the different parts of the brain. We train muscles by exercise, and so we may

ɜ different organs of the brain by proper exercise intellectual work, by strengthening the power of and by the regulation of our emotions. I think pro- ɜ highest of all our mental states, that which above guishes man from the other animals, is what may be e *power of the will*, or the exhibition of the faculty int. We begin our education really by being taught in ourselves. In the same way, as we go through are taught to curb our tempers, and to do our n at times when it may be disagreeable to do so. re is a physiological basis for this. That is what articularly to enforce. If we have special centres l with restraint, as I have pointed out, it is quite hat, by working these centres over and over again, ase their power, and consequently it becomes easier ı afterwards to restrain himself than it was at first. we go back, with regard to these matters, to the ritten many years ago by the wisest of men: "He th his spirit is better than he that taketh a city." olomon's wisdom the worst that could be said of a indicating failure and desolation, was, that "He not rule over his own spirit, is like a city that is own, and without walls."

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IS MAN AN AUTOMATON?

Ladies and Gentlemen,—In introducing to you the uestion which is to be the subject of my address this vening—the question, Is Man an Automaton?—it is erhaps well that I should define, at the commencement, he sense in which I intend to use these words; and it will e more convenient to take the second first—What do I nean by an Automaton? The word automaton is derived rom two Greek words, which mean self-moving. Well, of ourse, man is a self-moving being, and in that sense he is n automaton. But the word automaton, as we use it, has different signification. It means a structure which moves y a mechanism, and which can only move in a certain vay. I might take as illustrations various automata which re exhibited from time to time—I remember to have seen n my boyhood many remarkable collections. But I will draw my illustration from this very hall in which we ar met. The great organ behind me is blown, I understand, by water power. You know, I daresay, that formerly organs were blown by manual or human power. The bellows-blower had before him what is called a "tell-tale," a little weight so hung as to indicate the amount of wind in the organ; and his business was to work the bellows so as lways to keep the "tell-tale" below a certain point. On he other hand, by a piece of mechanism constructed for he purpose with a great deal of skill, the organ is now lown by water-pressure. The water-pressure so acts, that hen the organist requires a large supply of wind, as when e is playing loud through a great many pipes, the bellows

move faster and supply that wind; while, on the other hand, when he plays softly, and little wind is required, the bellows move more slowly. If that apparatus were incased in the frame of a human figure, and made to work the bellows-handle up and down, we should call it an automaton.

Now, let us see on what the working of that automaton depends. It depends, in the first place, upon its structure. The mechanist who has constructed that apparatus has so arranged the play of its various parts, that it shall work with the power communicated to it, in accordance with the organist's requirements. Then its working depends upon the force supplied by the water-pressure; that force being made, by the construction of the machine, to exert itself in moving the bellows at the rate determined by the playing of the organist. Without a sufficient water-pressure the machine will not work; and when the organist ceases to touch the keys, the movement of the bellows comes to a stand. There you have then a machine which is moved, on the one hand, by a certain power, and the action of which is regulated by another set of circumstances external to itself. Now that is, I think, what we mean by an automaton—a machine which has within itself the power of motion, under conditions fixed *for* it, but not *by* it A watch, for instance, is an automaton. You wind it up and give it the power of movement; while you make it regulate itself by its balance, which you can so adjust as to make it keep accurate time. Any piece of mechanism of that sort, self-moving and self-regulating, is an automaton. But then all these machines are made to answer certain purposes, and cannot go beyond. They are entirely dependent, first, upon their original construction, secondly, upon the force which is applied to them, and thirdly, upon the conditions under which that force is made to act. The question then is, whether Man is a machine of that kind?—his original constitution, derived from his ancestry, in the first place, shaping the mechanism of his body; and in the second place, the circumstances acting upon him through the whole period of his growth, and modifying the formation of his body, also, in the same manner, determining the constitution of his mind. Are we to regard the whole subse-